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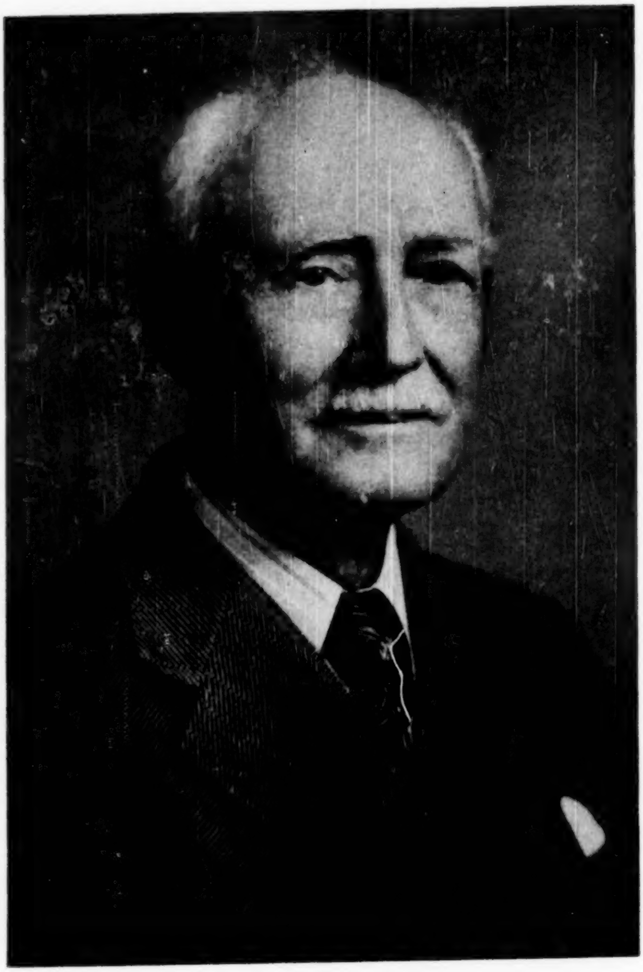

C. E. Seashore

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THE PSYCHOLOGICAL REVIEW

CARL EMIL SEASHORE

1866–1949

Carl Emil Seashore was born in Morlunda, Sweden, January 28, 1866. He died in Lewiston, Idaho, on October 16, 1949 at the age of 83. He came to the United States in the fall of 1869 with his parents and a younger sister. The family settled in Boone County, Iowa. His childhood was spent on the farm and his early education was typical of that of an Iowa farm boy in the '70s. Far from being an educational handicap, Seashore always considered this to have been an opportunity to develop self-reliance and skill in numerous fields. Any formal educational handicap was quickly and effectively overcome as Seashore went through college at Gustavus Adolphus and then, for graduate study, to Yale University.

From the beginning of Seashore's early country days, he seemed destined for a career in education. As he himself has summarized the matter in his "Open Letter to Seniors," published in 1912, "I took one year of a preparatory course for teaching in my own country school. I took another year to prepare a little better. I took three more years to prepare for teaching in a city school. I took another year to prepare better. I took another year to prepare for teaching in the university. I took two years to prepare better."

After receiving his doctorate at Yale University under E. W. Scripture in 1897, Seashore went directly to the University of Iowa as Assistant Professor of Psychology. He was rapidly promoted to higher professional rank, and became head of the Department of Psychology and Philosophy in 1905 and Dean of the Graduate College in 1908.

Dean Seashore's influence upon psychology and graduate study in America has been profound and extensive. The growth of the Iowa psychological laboratory under his leadership was phenomenal even during a period when growth and expansion were the order of the day. He served as president of the American Psychological Association in 1911. Of the many areas in which he was recognized as a pioneer, a few of the more outstanding are the psychology of music, attention to the "gifted student," clinical psychology and mental health (the Iowa Clinic was established in 1908), child welfare, mental tests, and the psychology of hearing, speech and art.

Seashore's work on the measurement of musical talent attracted the attention of George S. Eastman, who attempted to persuade him to leave the University of Iowa and come to the Eastman School of Music. Seashore felt that research in the field could be conducted more advantageously in the university laboratory. Mr. Eastman therefore subsidized the University of Iowa Eastman fellowships in the psychology of music which were used for many years to attract

promising research musicians to this field of study.

Seashore was a major force in the organization and early activities of the National Research Council; he served as chairman of the Division of Anthropology and Psychology of the Council in its third year.

Always a venturesome and fearless leader, Seashore blazed the way where the more timid were either too fearful or unimaginative to advance. His scientific and laboratory training under Scripture was well designed to yield a handsome return when applied to a field as virgin as psychology at the beginning of the twentieth century. Recognition was slow at the beginning but increased rapidly before he reached the age of fifty. It is typical of the man that he went contentedly back to his laboratory when the University of Iowa chapter of Sigma Xi refused to elect him to membership during his early days at Iowa because psychology was not considered to be a science. He was, of course, later elected to that society. He took in stride (though undoubtedly a rather proud stride) his admission to the National Academy of Sciences as a representative of psychology in the early part of the century. He was the first person at the University of Iowa in *any* field of science to be so honored.

Seashore and Mary Roberta Holmes were married in 1900. Mrs. Seashore played a highly important part in the developing career of the Dean. She helped immeasurably in building good will among students, colleagues, and citizens of the community. Entirely aside from Seashore's recognized work as a pioneer in psychology, Dean and Mrs. Seashore were truly an inspiring couple. Mrs. Seashore died on August 18, 1949, just two months before the Dean's own death. In a printed "letter to his friends," after Mrs. Seashore passed away, Dean Seashore expressed his essential optimism and faith in the future. This letter was not only a moving tribute to Mrs. Seashore but its very tenor was a magnificent expression of the philosophy which guided the lives of this couple for a half century. Psychology has lost a great and true pioneer in Carl Emil Seashore.

JOSEPH TIFFIN

Purdue University

SITUATIONAL ANALYSIS: A CLASSIFICATION OF ORGANISM-FIELD INTERACTIONS

BY J. L. FULLER

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Introduction

The stimulus for the writing of this paper arose from a survey of the methods to be employed in a long term study of the relationship between heredity and behavior. This relationship has interested biologists and psychologists for a long time. Its solution (if one can speak of the solution of so general a problem) will require the integration of theories arising from both groups, and experimental data secured by many different techniques. At the Roscoe B. Jackson Memorial Laboratory, the dog has been chosen as the most suitable subject for researches of this nature. The kinds of behavior shown by dogs are complex enough to be psychologically interesting; selection has produced numerous strains differing in structure and activity; and the physiology of the species has been widely studied.

To secure the maximum amount of information from a program devoted to the study of heredity and behavior, two plans of attack must be followed simultaneously. By studying many individuals in one situation, the experimenter determines the average performance and distribution of abilities within the population considered. Thus any individual may be ranked in comparison with the total population. The second plan is to give many tests to one animal, and correlate performance in any one test with that of another. In this manner one can determine whether there are general traits or factors operating throughout a wide range of situations.

Researches in behavior may be classified according to the preponderance of interest in general laws or in individuality. A psychologist studying maze learning in a rat is ordinarily more interested in proving or disproving a theorem of learning than in understanding the causes of variability in his subjects. For his purposes minimum variability is very desirable. This would also be true of a biologist investigating the effect of a nutritional deficiency on maternal behavior. The efforts of the student of heredity must be directed towards a study of individual differences, and an attempt to explain these differences by means of the least number of independent variables. Individual differences are the raw material of genetic research and must be looked for in all important realms of behavior. A set of general concepts covering all possible types of situations is needed for the guidance of the investigator in designing tests and recording observations. It is the purpose of this paper to discuss a scheme which is proving useful in orienting behavior research on animals in a variety of specialized situations, and which should be of interest to psychologists and other students of animal behavior. The name "situational analysis" is appropriate, since it involves a classification of test situations according to the characteristics of the experimental field, and considers the animal's response in terms of adaptation. The scheme is broad in scope, and attempts to include all the organism-field interactions which are significant for behavior. These interactions are discussed below in general terms only, but the concepts can be set up in a form which permits quantitative prediction followed by experimental verification.

DEVELOPMENT OF THE IDEA OF SITUATIONAL CLASSIFICATION

One of the early problems arising from the study of dog behavior was the interpretation of performance tests. The failure of an individual to acquire a new mode of behavior during training might be due to a lack of motivation or a lack of specific learning ability. To distinguish between these possibilities, it is customary to manipulate the experimental field. If an increase in incentive improves performance, motivation is considered to be a critical factor. If a decrease in the complexity of the field improves performance, then ability is the limiting factor. To measure motivation by itself, one must arrange a situation of low complexity and systematically vary incentive.

To measure ability in "pure" form, one must maintain motivation at an adequate level, and vary complexity. These relationships have been discussed in relationship to the psychometric analysis of human personality by Cattell (1). It is possible to describe any test situation in terms of its incentive value and complexity. Each of these parameters can be considered to influence behavior differently in the low, middle, and upper portions of its range. At low levels of incentive, an animal shows undirected, low-energy reactions. At middle levels the response increases as the value of the incentive increases, while at high levels, increase in incentive no longer has any effect since the maximum performance has already been attained. Low levels of complexity permit immediate attainment of the incentive. At middle levels the animal's performance is inversely related to complexity, while changes of complexity above the subject's upper limit will have no effect on performance. These relationships are represented diagrammatically in Fig. 1.

This diagram is useful in designing experiments and in the interpretation of many customary procedures used in behavior studies. It has two important defects, however. In the first place it gives too little attention to the

High Optimal	No further improvement in performance even though incentive is increased.	Performance is dependent upon complexity of the situation, since motivation is always adequate and essentially constant. These conditions are necessary for measurement of ability.	A combination of very high incentive and an insoluble problem produces maladaptive behavior.
Moderate Incentive	Performance improves as incentive increases. Complexity is at so low a level that it does not interfere with measures of motivation.	Performance is dependent upon both incentive and complexity, and the effects of these two factors cannot be easily separated.	Insoluble problems. Useful only as a measure of finding a limit to a subject's learning ability.
Low	No motivated behavior Training impossible Random, low-energy-level behavior		
	Low	Moderate Complexity	High

FIG. 1. The original incentive—complexity diagram.

importance of the organism's interaction with the field. Not incentive alone, but an interaction between some object in the field (incentive) and a particular organism in a particular state determines that organism's behavior. Similar considerations apply to the ability-complexity relationship. Secondly, the term motivation as commonly used includes two distinct concepts, that of directing behavior towards a specific goal, and that of mobilizing energy to respond. It seems better to separate these two aspects of motivation in an objective description of behavior. An account of a particular behavior sequence can be reduced to statements regarding three aspects of the behavior: (1) the general direction (selection of goal); (2) the utilization of cues to select a specific path; and (3) the amount of energy mobilized for motor activity (including covert functions such as changes in muscle tension and blood sugar, as well as gross body movements). In addition, behavior is influenced non-specifically by the general level of basal metabolic processes. This framework for the description of behavior, though derived independently is essentially the same as the scheme proposed by Duffy (2). Obviously the inclusion of two additional parameters has produced a set of relationships more complicated than those of Fig. 1, but more adequate for the task at hand. The following sections present: (1) the basic concepts of organism-field interactions which are used in classifying situations; (2) a descriptive classification of situations; and (3) a brief account of some of the applications of situational analysis.

SITUATIONS AS DETERMINANTS OF BEHAVIOR

The basic postulate of situational analysis is that behavior is a form of interaction between an organism and its environment. When the environmental field conditions are exactly specified, and the organism is in a given physical state, it will respond in a determinate manner.[1] Since different types of situations produce new interactions, behavior will vary qualitatively as an animal encounters these new conditions. Obviously the total number of situations is infinite, and the possible states of an organism are extremely numerous. The purpose of classification is clarification by setting up a smaller number of categories into one of which all possible situations can be assigned.

In developing a classification of organism-field relationships, an effort has been made to avoid basing the concepts on controversial theories. To be sure, it is imprudent to build a conceptual structure without foundation in fact and theory. Although learned responses are extremely important in determining particular organism-field interactions, the current controversies regarding the nature of learning appear to be inconsequential for our purposes. It should be kept in mind that situational analysis is a cross-sectional concept and that it must be supplemented by longitudinal concepts such as learning and maturation.

The Field Parameters

In describing the behavior of an animal one must specify the rate of energy expenditure (covert as well as overt) and the direction in which this energy is being applied. For a given organ-

[1] There is much merit in the statement by Freeman (3) that behavior should be described in terms of intra-organismal occurrences. Thus it is sensory processes which directly influence behavior—not a vague "field." However, most experimenters manipulate the sensory processes by modifying the "field," while technical limitations make it impossible to quantify the neuro-sensory phenomena.

ism in a given state these are determined by three specific parameters and a number of non-specific factors of the field. The specific qualities are: (1) field incentive, (2) field impedance, and (3) field complexity. The general factors include such environmental characteristics as temperature and humidity, which affect energy mobilization in a non-specific manner. Under certain circumstances such non-specific characters may become specific incentives for a behavior sequence. Each of the three specific parameters will be discussed briefly.

An *incentive* is a goal object towards which a behavior sequence is directed. Examples are food, a sexually receptive female, or a predator which produces an avoiding response. When the goal is attained (or in the case of negative incentives removed from the field), the behavior sequence ends. Behavior sequences may be interrupted by external factors before goal attainment or avoidance is complete. For any one subject it is possible to set up a scale of valuation based upon a comparison of its performance in a situation where incentive is the only variable, or upon the subject's preference for one or another incentive. Quantitative attributes such as the amount of food, or qualitative ones such as food versus water may be utilized.

Field *impedance* depends upon the presence of obstacles between the organism and the goal. Examples are excessive distance, a hurdle in the goal path, or another animal competing for the same goal. The limit is reached when the goal is inaccessible.

Field *complexity* varies from situations in which the path to the incentive can be directly perceived to those in which there are no reliable sensory cues. In the intermediate stages of complexity proceeding towards the goal depends upon selecting from several sensory cues, or choosing a particular sequence of cues along the path. Cue complexity puts demands upon the animal's sensory system, including receptors and their associated central structures.

The Organism Field Interactions

Since behavior is always an interaction of organism and field, the qualities of the field alone do not determine the type of behavior. The interactions of an organism with its environment are, of course, very specific in many instances. Such specificity requires explanation, either by reference to the past history or to inherited determinants of the organism, but it is not within the scope of the present paper to do so. Corresponding to each of the three field parameters is an organism-field interaction. The quantitative expression for each of these is selected so that unity represents the highest possible value for the interaction. The best possible performance on any given test for any subject is its performance potential. This has the characteristics of an interaction but the units chosen to express it will vary according to the type of test.

Directive Interaction (*D*). The resultant of the organism-incentive interaction may be called the *directive interaction.* The intensity of this interaction is defined as the ratio of the energy output applied in goal-directed behavior to the total energy output during the time period considered. Energy requirements for basal metabolism should be subtracted from both terms. This ratio is independent of the absolute energy output. Thus, inhibition of activity in response to a stimulus may represent a strong directive interaction, since the energy which is being expended is controlled by the incentive. It is convenient to postulate a property of the organism, directability, which is

a measure of its capacity to interact with a class of incentives (**5**).

Directability obviously depends upon an animal's physiological state. Thus, meat is a poor incentive for a satiated dog, and a bitch in heat does not attract a male castrated as a juvenile. Objects with no primary incentive value may have it conferred upon them by learning. Although this concept of directive interaction implies a continuum from zero to one, we can distinguish, as was done on the simpler chart, three qualitatively different ranges. These ranges are (1) inadequate, (2) adequate, and (3) supra-optimal. At the lowest level no directed activity is evident. An observer might say colloquially, "The animal is doing nothing in particular." Skinner's (**7**) concept of "emitted behavior" appears to be very similar to this. When directive interaction is adequate, behavior is definitely goal directed. In this range, the cue complexity and goal accessibility of the field become important determiners of performance. The term "supra-optimally directed behavior" applies properly only in complex situations where some inhibition of goal directed activity is necessary for a successful performance. This will be discussed further in another section.

Cybernetic Interaction (*C*). The directive interaction determines the degree to which an organism is oriented to a specific goal. Another interaction is necessary for efficient selection of the path to the goal. A term recently introduced by Wiener (**8**) is applicable to describe this type of interaction, and we shall speak of it as the *cybernetic interaction*. This refers to the selection of cues which will direct the organism most efficiently towards the selected goal.

The adequacy of cybernetic interaction in any particular situation may be measured by the frequency of occurrence of "errors of direction," errors which involve taking a path which is not the most efficient. Indirectly it may often be determined by the time needed to reach the goal from a given starting place. A numerical measure of this interaction is given by the term $(1 - e_d/N)$ in which e_d represents the number of "errors of direction," and N is an empirical constant derived for each particular situation.

The selection of cues which will direct behavior toward the goal is assigned in this discussion to an analyzing function of the organisms. This involves perception of stimulus patterns which differentiate "best" paths from other paths. The nature of the perceptive and analyzing processes is the subject matter of Gestalt psychology. There are three qualitatively different levels of interaction. At the simplest, we have direct sensory perception, and e_d will ordinarily be zero. An animal smells food and has merely to follow the gradient of odors to approach the goal. At the second level of complexity the selection of one or more preliminary cues are required to direct activity towards the goal. A finite number of errors of direction will occur. As a result of learning, the number of e_d's is reduced, and the situation may approach one of a directly perceived path. This reduction in errors of direction is one of the chief interests of students of learning.

Finally we have situations in which there are no reliable sensory cues indicating the direction to the goal, and there is no reduction in the number of e_d's as a result of experience.[2] In cases of this kind the incentive itself is not perceived, but directed activity can occur nevertheless. An example is the activity of animals accustomed to being fed at a particular time each day. Pre-

[2] For exceptions to this generalization see the discussion below of sector 231.

paratory feeding behavior originates before food can be sensed in any way.

Performance Potential (K). In describing behavior one must specify the power output as well as the direction. Performance potential refers to the total power resources and motor apparatus available to the organism. Living organisms require continuous energy output to maintain their organization, so that metabolism cannot fall below a minimum without producing irreversible changes. Individuals differ in basic metabolic output to some degree, but vary much more in their habitual level of total energy expenditure.

At high levels of performance potential, latencies of response are small, speed of performance is high, and great force is available to oppose resisting forces. At low levels less energy is available and responses are slower and less forceful. Latency of response, speed of response, and force are useful in measuring this quantity, which can vary indefinitely from the basal level to any practicable upper limit. Individual differences in K in animals are ascribable to metabolic factors such as endocrine balance and nutritional state. Within an individual phenomena such as sleep and fatigue periodically alter K. At an inadequate level of directive interaction, behavior such as visual scanning and position adjustment may be considered as expressions of undirected performance potential.

The Effective Interaction (E). Power alone will not assure effective behavior unless it is adjusted to the impedance of the field. An efficient organism adjusts its power output to resistance by an organism-impedance interaction. It is convenient to postulate a motorability function of the organism which is high in individuals which make few errors of effect (e_e). An error of effect occurs whenever the organism attempts to follow the most direct path to the goal, but fails because of too little or too great expenditure of energy. The numerical value of E is given by $1 - e_e/M$, which is strictly comparable to the expression for cybernetic interaction. Not all errors of effect are overt. Autonomic responses are part of the energy mobilization included in the effective interaction. If these are inappropriate to the actual situation, they constitute an error of effect.

The effect of differences in field impedance upon behavior depends upon the motorability of the particular animal. Three ranges of interaction producing qualitatively different types of behavior may be recognized. (1) The direct path includes no serious obstacles between the organism and the goal, nor is any unusual motor dexterity or energy mobilization required for success. There are no errors of effect. (2) In the obstructing path there are obstacles to be surmounted or very precise motor acts to be performed. A finite number of e_e's occur. (3) An impassable path implies an inaccessible goal, and the number of e_e's will increase indefinitely so long as the animal persists in the same direction. It must be emphasized that barriers which are impassable at an early age may be overcome later as an animal develops strength and agility, and become impassable again during senility. Barriers are not necessarily static, but may include the speed of a movement of another animal, or the presence of another organism competing for the same goal.

An Explanatory Example. It is possible that the above description may be clarified by means of an example. Let us consider a dog released at a distance from its home, but in familiar territory. The journey from the release point to its home is the behavior sequence under consideration. Directive interaction occurs between the animal and its destination, which in this case cannot be directly perceived. If the animal makes the journey by the most

direct route, then directive interaction is high. If many side trips are involved, directive interaction is low. Cybernetic interaction is represented by following trails visually and sniffing at posts. The animal may take a wrong turn and commit an "error of direction." The distinction between errors of direction and side trips due to low directive interaction is not arbitrary. Increasing directability of the subject (hunger would be effective in this respect) will eliminate side trips, but not necessarily influence errors of direction.

Another possible source of confusion between the directive and cybernetic interactions lies in the improper selection of the behavior sequence. One might consider the release point to home journey as a series of point-to-point journeys, each point being a goal involving directive interaction. The test for this interpretation is the insertion of low impedance bypasses around both way-stations and destination. Given an opportunity to learn both the long way, and the bypasses, the organism will adopt paths eliminating points reacted to cybernetically, but not those reacted to directively. The same physiological mechanisms are involved in both interactions, but the response to changes in the situation serves to differentiate them. They could not be distinguished on the basis of a single trial run.

The performance potential of the organism is determined by such physical factors as length of limb, and cardiovascular and respiratory efficiency. Environmental factors, such as high temperature which interferes with heat elimination will affect his performance. So may internal factors such as blood sugar concentration. The effective interaction is represented by response of the dog to fences, rivers or hills along the way. An "error of effect" may result from improperly gauging the height of a fence, or the width of a stream. Errors of effect may also occur as a result of lowered performance potential due to low blood sugar. Externally these could not be distinguished from a failure produced by clumsiness. A physiological study is necessary in order to distinguish between these two possibilities.

Given a particular objective, cues, physical power, and control of this power, the behavior of the dog is a function of the situation, the odors, barriers, and visible cues found along the goal path. The demands upon each interaction systems likewise varies in different situations—a single track, wilderness trail may afford no opportunities for errors of direction and innumerable ones for errors of effect, while a well paved, heavily travelled city neighborhood favors errors of direction.

CLASSIFICATION AND DESCRIPTION OF THE INTERACTION SECTORS

We have considered three ranges of three organism-field interactions, which are of particular interest. The 27 possible combinations may be reduced in number, since under some conditions the influence of certain field parameters may be neglected. Each possible combination of these is a *sector,* and is assigned a three digit number representing in order the level of the directive, cybernetic, and effective interactions. Variation in performance potential will affect behavior quantitatively in all sectors, and may have a qualitative effect in special cases.

The levels considered are:

Directive (D)	Cybernetic (C)	Effective (E)
1. Inadequate	1. Direct perception of goal	1. Direct access to goal
2. Adequate	2. Complex cues to goal	2. Obstructed path
3. Supra-optimal	3. No reliable cue	3. Impassable path

Figure 2 represents the relationships of these variables diagrammatically. Any situations may be assigned to one of the blocks in the inverted pyramid. The outside blocks are numbered according to the system described above.

In describing behavior in the various sectors, the term "adaptive behavior" is employed. Adaptive is used in the limited sense of referring to responses which are most efficient in attaining the goal, or in preventing expenditure of energy directed towards an unattainable goal. In a broader sense, "adaptive behavior" must include reference to the choice of incentives when several are presented simultaneously, and may be extended to include behavior which is adaptive for society though not for an isolated individual.

Adaptive behavior involves a harmonious interplay of interactions. In the terminology used in this paper, the optimal value for the cybernetic and effective interactions is unity. This signifies the absence of errors of direction and effect. The optimal value for the directive interaction varies according to the situation and this interaction has, therefore, been used as the primary heading for classification of situations.

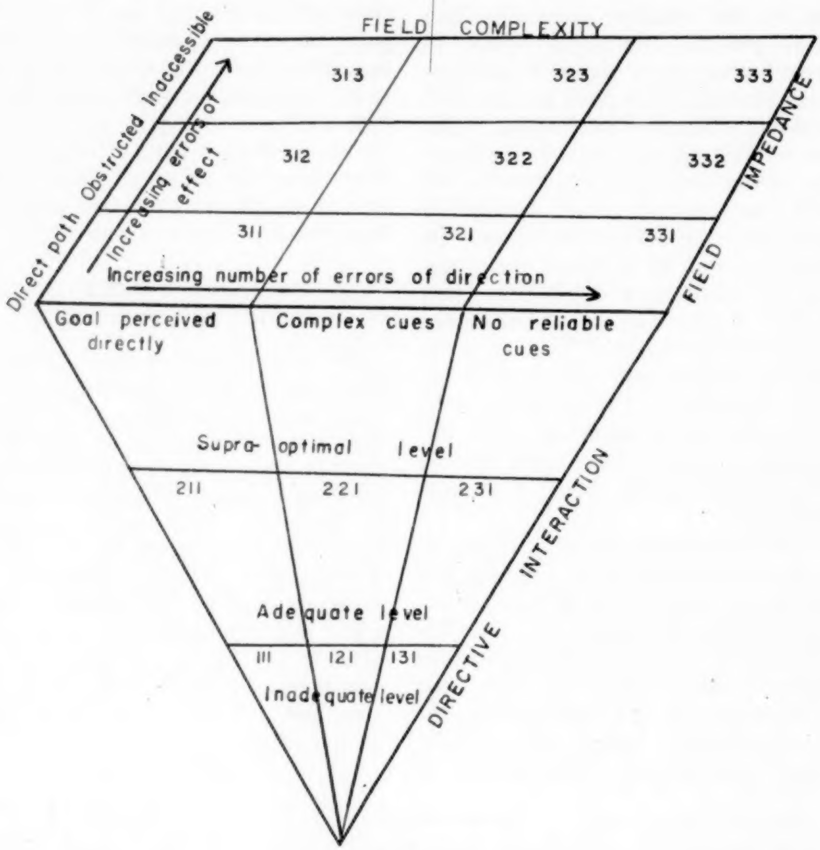

FIG. 2

	Direct perception of incentive	Preliminary cues for guidance	No reliable cues
Goal Inaccessible	Attempts to reach goal prove a failure. Behavior soon directed to another incentive. 213.	Problems of sensory complexity with inaccessible goal. The adaptive behavior involves extinction of a learned response. Situations of this type exist only for a trained subject. 223.	Insoluble problems, with inaccessible goal. Situations of this type are useful only to determine limits of abilities. Adaptive behavior is exploration followed by extinction. 233.
Impeded Path	Immediate approach at a high power level. The energy output is accurately adjusted to actual requirements. Problems of motor difficulty, but sensory simplicity. 212.	Problems of combined motor and sensory difficulty. Success depends upon learning cues, and new motor pattern. Various solutions are attempted and the successful one adopted. 222.	Problems of motor difficulty without environmental guides. Such problems will usually be insoluble, since there is no directly perceived incentive to direct activity across the path impediments. Chance solutions are possible. Adaptive behavior involves learning a plan of attack—not an association. 232.
Direct Path	Immediate approach. Behavior approaches reflex or tropistic simplicity. Very simple problems. 211.	Problems of motor simplicity and sensory difficulty. Efficiency depends upon learning cues. Various solutions are attempted, and the successful one adopted. 221.	Problems of motor simplicity without environmental guides. Adaptive behavior consists of vigorous exploration. Improvement with time must involve learning type of attack, not a specific association. 231.

FIG. 3. Characteristics of adaptive behavior in the nine behavioral sectors at adequate level of directive interaction.

Direct approach to his master is not adaptive behavior for a dog if it involves plunging into a stream of traffic. A division of energy between the motor pathways involved in running to the goal and those used in scanning the environment for dangers will insure a longer life for the dog. On an isolated farm the direct approach is not as hazardous, and impetuosity is not penalized. Differences in performance potential have little adaptive significance until a stress is imposed. In general, a greater amount of available energy is advantageous, though in times of food scarcity a low level of metabolism has survival value.

Inadequate Directive Interactions: Sectors 111–133. At the level of ineffective D, behavior is random and the animal does not respond in an oriented manner towards any object. Since there is no goal, such concepts as goal accessibility and cue complexity do not apply. Differences in the kind and amount of random behavior may be ascribed to performance potential (K). It may be objected that no behavior is actually random, since some stimulus must be involved. The point to be emphasized is that the observer should postulate directedness only when it is present long enough and at a level high enough to be definitely related to some feature of the

field. The existence of individual differences in the activity of dogs during "rest periods" has been shown at this laboratory (9). Periods of random, low-level activity occur when there is no discernible incentive in the field. This behavior is similar to that shown by decorticate mammals. Individual and breed differences in the quantity of such behavior appear to be due to differences in internal conditions related to energy mobilization level.

Adequate Directive Interaction: Sectors 211–233. Each sector is described in a brief paragraph which defines the conditions, gives one or two examples, and describes adaptive behavior in the situation. Figure 3, which represents a cross-section through the middle of the pyramid of Fig. 2, summarizes the characteristics of these nine important sectors.

211. The goal is directly perceived and may be reached directly. An example is that of a hungry dog in the presence of food. The animal goes directly to the food and eats. The speed and force of its reactions are measures of directability and of performance potential.

212. The goal is directly perceived, but obstacles exist so that a considerable amount of energy, or quick and precise motor responses are required to attain it. Competitive feeding is an example of such situations, food is presented to a group of animals in such a way that all cannot feed at once. Domination over competitors allows one animal to secure the food and represents adaptive behavior in this situation. Subordination and stealing scraps of food represent a type of adaptation for an animal of relatively low performance potential.

213. The goal is directly perceived but cannot be reached. Food may be presented just out of reach, or covered. The animal approaches it and makes vigorous efforts to reach it. The behavior sequence is rapidly extinguished by failure.

221. There are no physical obstacles blocking the path to goal, but proper cues must be selected if it is to be reached efficiently. An example is a simple place or sign discrimination experiment. Adaptive behavior differs for trained and untrained animals. The untrained animal explores until the goal is reached. The correct solution can occur by chance on the first trial, but this is unlikely. After training the animal goes directly to the goal.

222. Both cue selection and motor ability are required to reach the goal efficiently in this type of situation. A barrier type maze, a discrimination problem requiring a specialized motor performance, and the establishment of a social hierarchy in a group of animals are examples. An untrained animal tries various solutions, makes many errors of direction, and may reach the goal accidentally. On the other hand, it may fail and cease activity at one of the "barriers" because of an error of effect. The trained animal moves more directly to the goal. For the fully trained animal, a situation of this class becomes equivalent to sector 212, or even 211, as errors of direction and effect are eliminated.

223. This type of situation is of the same degree of complexity as the preceding one, but differs in having an inaccessible goal. The arrangement is that of experimental extinction of a learned response. This type of situation does not exist for the inexperienced subject. If by chance it comes close to the goal, attempts to reach it should be rapidly extinguished, as in sector 213. The trained animal will make a direct approach. Failure to actually reach the goal will be followed by extinction.

231. In situations of this type there

are no physical obstacles to immediate attainment of the goal, but there are no consistent cues as to its presence or direction. For example, food may sometimes be available in one of several places, but no sign is available to indicate which one is correct on any particular trial. In such a situation training for a particular solution is impossible. Adaptive behavior consists in trying all possible solutions until success is attained. Improvement in performance involves learning a mode of attack, not an association between a specific cue and the goal. Thus the number of e_d's may go down, even though it can never reach zero.

232. The goal is concealed with no cues to location. In a situation of this type success will come only by chance. In situations involving considerable motor difficulties or a large field with many places of concealment, the probability of reaching the incentive is very slight. If the animal should be successful, adaptive behavior is an attempt at the same solution. This is followed by extinction of the response, since the cues are not the same from trial to trial. Other solutions may then be attempted. Adaptive behavior involves learning a method of exploration—not an association between a specific cue and goal. The organism must generalize kinds of places which may conceal the incentive.

233. Situations in this sector are insoluble in both sensory and motor aspects. They cannot, therefore, be profitably employed in behavioral studies, except in so far as insoluble problems may be set up to find the limits of an animal's abilities. The only possibility is that an animal will have near-success by accidentally approaching and perceiving an incentive. The adaptive response is to attempt to secure it, and for extinction to occur following failure.

Supra-optimal Directive Interaction: Sectors 311–333. Directive interaction was defined above as a measure of the proportion of total energy directed towards a particular incentive. The term supra-optimal is not appropriate for all sectors, for in some situations such directness of approach is an adaptive form of response. A high level of D prevents extinction, so that an animal persists in attempting to reach an inaccessible goal. In cases where adaptive behavior involves the learning of specific cues or a complex motor pattern, a supra-optimal level of D opposes the elimination of goal directed behavior which proves inappropriate. Such situations frequently require the animals to turn away temporarily from the goal in order to find the most efficient solution, and by the definition above such action can only occur at a low level of D. It follows that the optimal level of D for the solution of any problem is a function of the directness of the path. Many situations involve problems which are insoluble by association-type learning—in other words there are no reliable cues. A high level of D will neither add to nor detract from the possibilities of initial success. When success comes as the result of random exploration, the supra-optimally directed animal tends to persist in the originally successful solution, even though it is inappropriate. Such solutions are "superstitious." The animal reacts by a directive interaction to constituents of the field which should be reacted to by a cybernetic interaction.

The nine sectors to be considered are physically the same as the sectors described above. The problems are the same. Adaptive behavior is, therefore, the same. The level of D is greater because of greater motivability of the subject, or the increased value of the incentive. Extreme deprivation, or a period of learning producing a strong

directability in the subject, is always a requirement for this state.

Sector 311. Direct path and direct sensory cues. A high D does not hinder performance, which is equal to or superior to that of the subject at a lower level.

Sector 312. Straightforward sensory cues, but motor difficulty. Performance may be very efficient if a directed power output is the chief requirement. Even complex motor processes, if well learned, may be carried out well.

Sector 313. Direct sensory cues, but goal inaccessible. The supra-optimally directed animal shows unadaptive behavior, since extinction of goal-directed behavior does not occur promptly.

Sector 321. Goal is readily accessible, but discrimination of proper cues must occur if behavior is to be most efficient. The supra-optimally directed animal learns slowly since its behavior is primarily a function of incentive interaction, not of cybernetic interaction.

Sector 322. When sensory and motor difficulties are combined, the supra-optimally directed animal learns slowly for the reasons stated above.

Sector 323. In situations of this type, complex sensory cues direct only to a position near the goal, which is itself inaccessible. Only a trained animal can be brought into this sector. Supra-optimal D prevents the adaptive response of extinction of the goal directed behavior.

Sector 331. Goal is easily attained so far as motor processes are concerned, but there are no cues as to its location. A supra-optimally motivated animal with high performance potential may be very effective (though inefficient) due to the vigor of its exploratory behavior. It will tend to fixate on a particular solution which is found effective.

Sector 332. The addition of motor difficulties to problems involving no reliable cue makes them more difficult, but does not change the nature of adaptive behavior, nor the variant type shown by supra-optimally motivated animals. See sector 331.

Sector 333. Situations of this type are insoluble, and as stated above (under Sector 233) they furnish useful data only as indicating the limits of particular subjects. Adaptive behavior is simply to leave the situation if possible. Only an animal trained in similar physical situations where the goal is accessible will perform. The supra-optimally directed subject may continue exploratory behavior for an uneconomical length of time.

Applications of Situation Analysis

The applications of this group of concepts are numerous. For reasons of space, and because some have not yet been worked out in detail, only a few applications will be discussed here. One important use is to relate different experimental arrangements to each other. The controllable variables of the field are incentive, field impedance and cue complexity. The animal subject is placed in this field and observations of his performance are made. Such observations may include: (1) the number of times a certain act is performed in a unit of time; (2) the latency of a response; (3) the amount of time required for a particular behavior sequence; (4) the deviations made from the most efficient path (errors of direction); (5) failures to accomplish an attempted action (errors of effect); (6) or the force applied in a given act. From these performances the experimenter endeavors to estimate the properties of the organism. In this paper these properties are considered to be motivability, motor ability, cue analyzing ability, and performance potential. To some extent, performance always depends upon the three modes of interaction described above, as well as upon

performance potential, which interacts with the environment in a much less specific way. Frequently the experimenter wishes to arrange conditions so that a relatively "pure" measure of one property of the organism is obtained.

There are two general types of comparisons to be made. Inter-individual comparisons are based upon the performance of an animal at one time with his performance at another. If only one aspect of the field is varied at a time, and if enough observations under each set of conditions are made to allow for random variations of the supposedly constant interactions, then the performance of an animal may be taken as a measure of one of its properties. Even here difficulties of interpretation arise if the relationship is not linear. Intra-individual comparisons are very hazardous, unless the influence of some interactions is minimized by the proper arrangement of the situation. A paragraph will be devoted to the measurement of each of the four organismic properties, directability d_o, performance potential k, motorability e_o, and analysis-ability c_o.[3]

Measurement of Directability. The measurement of directability must be made under circumstances in which differences in e_o and c_o do not affect performance, and k_o is constant. There is no sure way to accomplish the latter, but differences are reduced by using animals of uniform heredity, reared under uniform conditions. Performance potentials can be placed on equivalent scales by using the best performance of each subject as the indicator of its potentialities, and scoring each individual in relationship to its own best score. If subject A performs an act 30 times in one hour under maximal incentive, and subject B performs the same act 60 times in an hour with maximal incentive, ${}_Bk_o = 2{}_Ak_o$.[4] When equally directed, A will perform only half as effectively as B. The sectors in which differences in e_o and c_o are unimportant to performance are: (1) those in which there is direct perception of incentive and no motor difficulties, sectors 111, 211, 311; (2) those in which the goal is inaccessible to all subjects, 113, 213, 313. (Sectors 132, 232, and 332 are equivalent for trained subjects.) Sectors with no reliable cue appear to be unuseable, since chance plays so large a part in success.

Measurement of Motor Abilities. These abilities are best determined in sector 312, in which directive interaction is at its highest level, the path is obstructed, but directly perceived. Complex motor abilities frequently require a well developed ability for analyzing complex cues, so that they cannot be isolated in pure form. Situations of this sort are responded to more adequately by animals at the intermediate level of directive interaction.

Measurement of Analyzing Capacity. Cue analyzing ability (c_o) is frequently called intelligence. Extremely careful control of the directive interaction is essential. A high D is essential to obtain good performance, but supra-optimal D disturbs learning. It is difficult to hold D at a high level without exceeding the optimum. The path must be unimpeded so that motor ability does not influence the performance. The presence of obstacles in early stage of learning may produce an extinction of all goal-seeking behavior and retard learning. Only sector 212 in the highest level of D possible without going over into the supra-optimal sectors fulfills these requirements. The difficulty

[3] Lower case letters with appropriate following subscripts are used to designate the contributions of organism and field to the four interactions described. Thus D = Directive interaction; d_f = incentive value of field; d_o = directability of organism.

[4] Subscripts preceding the interaction terms refer to specific organisms or to specific fields.

of achieving these conditions is reflected in the great variability of performance shown by most animals in "intelligence tests."

Measurement of Performance Potential. Performance potential (k_o) is the limiting factor for effective behavior in all situations, and is defined as the performance when there are no errors, and all energy is applied in goal directed behavior. Certain types of individual physiological and behavioral differences show up most clearly under conditions of minimal stimulation or when the goal is inaccessible. Since the kind of behavior shown in these situations appears to be based upon physiological factors, and these determine k_o, one would expect to find significant relationships when these fields are explored.

The principles discussed above have been recognized by competent workers and utilized in the design of experiments. However, experiments in which "pure" types of interaction are sought cover only a few of the situations in which animals must function. One cannot expect to find orderly results in those situations where small changes in any one of the interactions can cause a fluctuation in performance. Behavior appears chaotic when it is sensitive to more than one of the determinants at once. For this reason such sectors are avoided by experimenters. Yet it is in these sectors that the most interesting and vital problems arise. It is believed that the viewpoint of this paper will be useful in deriving adequate conceptual and mathematical tools for the job. Preliminary studies indicate that the concepts can be formulated in precise symbolic terms capable of experimental verification, if one sector is considered at a time. A general equation applicable to all sectors has not yet been derived. Whether it is possible or not must await experiments and analysis of their results.

The relationship of these views to learning theory is that two major types of learning must be distinguished (Cf. 6). One type may be called incentive learning and results in the substitution of new goal objects for old ones. This changes the level of D in a particular situation. Instrumental conditioning is an example. The second may be called ability learning and results in greater ability to analyze cues, to overcome obstacles, and to mobilize energy in accordance to field conditions. This type of learning affects C and E. Although the examples in this paper have been drawn primarily from observations on the dog—a mammal fairly high in the complexity of its behavior—, the scheme appears to have usefulness in dealing both with simpler species and those which are more complex. In the former case, some of the sectors drop out since they have no application to organisms low in the psychological scale. If one were to apply this scheme to a very complex behavior pattern, such as that of man, it might be necessary to subdivide some of the sectors to a greater extent.

Summary

Situational analysis is a system of classifying the relationships between an organism and its environment which determine its responses as a whole. The unit of behavior which is subjected to analysis is a chain of actions directed towards a goal. The characteristic of directiveness as an essential feature of the activity and evolution of living organisms has recently been stressed by Lillie (5). The strength of directive interaction is measured by the ratio of directed energy expenditure (less basal) to total energy expenditure (also less basal). This can vary between zero and one. In any particular situation directive interaction may be considered as the product of the directability of

the organism and the incentive value of the goal.

Cybernetic or steering interaction occurs as the organism reacts to environmental cues which enable it to select the most efficient path to a goal. In situations of low complexity there is no distinction between cybernetic and directive interactions, since the goal is perceived directly. In complex fields the efficient organism must select from various apparent paths the particular path which is most suitable. Deviations from the most efficient path constitute errors of direction and indicate imperfect cybernetic interaction. Organisms with high analysis ability relative to field complexity make few such errors.

Effective interaction is involved in the mobilization of energy to meet the specific requirements of a situation. The amount of energy required depends upon the presence of obstacles between the organism and the goal. Too little energy or too much energy may result in errors of effect which are manifest in motor inadequacy rather than in the selection of a wrong path. The number of these errors is a measure of the relationship between field impedance and motor ability.

As an animal gains experience in a situation there may be a tendency for these interactions to become more efficient. The subject is directed more strongly towards a definite goal, and errors are reduced. The limit of performance when all errors are eliminated and directive interaction is at a maximum is the organism's performance potential. This will differ from one organism to another because of anatomical and physiological factors, and in the same organism from time to time because of oscillations and secular trends in physiological state.

Situational analysis thus provides for objective description of behavior in terms of definite types of interaction. It is a concept of great generality and may be applied in such fields as ability testing, comparative psychology and abnormal behavior. The formulations of the three interactions have been developed in a manner which renders the concept accessible to experimental test. The quantitative aspects of situational or interaction analysis have been touched on lightly in this paper, so that a complete justification of the above statement must be deferred for a later communication. It may be of interest to briefly compare the viewpoint of this paper with that of Hull's ***Principles of Behavior*** (4). Hull's system deals in general with stimulus-response relationships, and postulates various intervening variables to account for the observed quantitative correlations. Situational analysis starts from the more general concept of biological adaptation and views each situation as a challenge to the organism's integrity, to be met by interactions of the directive, cybernetic, and effective types. The nature of the interaction is dependent upon the properties of the situation and of the organism at the particular moment considered. The properties of the organism are greatly influenced by its past history, but physiological explanations of behavior must deal with the actual organization which is present rather than interpret behavior in terms of past experience.

The application of situational analysis to problems of classification has been developed at length in this paper. The primary basis of classification is the level of directiveness (or absence of randomness) shown. A subdivision is then made according to the demands made upon the cybernetic and effective interaction in order to complete the directed sequence of action. Three levels of directive interaction are recognized: inade-

quate, characterized by random behavior; adequate, characterized by definite goal directed behavior; and supra-optimal, in which behavior is so narrowly channeled that adaptation to changing situations is difficult. Similarly there are three levels of demand upon cybernetic and effective interactions. At the lowest level these demands are so slight that the goal directed sequence can be completed immediately. In the intermediate range the problem is soluble, but errors are made in the process. At the uppermost level of difficult no real solution is possible. Each possible combination of these levels is considered as a "behavior sector," and presents a distinct problem to the organism. An important difference between excessively high field impedance and excessively high field complexity is that the behavior sequence may be completed by chance in the latter case, although the problem has not been really "solved." Within each sector it is possible to state definitely the most adaptive type of organismal response, and to evaluate deviations from ideal adaptation. In some cases the ideal response depends upon performance potential as well as upon the properties of the situation. A fundamental deduction which follows from the above arguments will serve to summarize the objectives of the classification of situations, and will furnish a theorem to be tested by field and laboratory observation.

If the interaction demands of two situations are similar, the nature of the adaptive responses will be fundamentally alike no matter how different the situations appear physically or how different the behavior patterns appear externally. Thus, assigning a particular situation to one of the sectors described above provides an objective basis for comparing a subject's behavior in this situation with his behavior in other situations, and clarifies the problems involved in comparing the behavior patterns of organisms of different levels of neurological organization.

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NEED-REDUCTION, DRIVE-REDUCTION, AND REINFORCEMENT: A NEUROPHYSIOLOGICAL VIEW [1]

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The chief classes of observed phenomena upon which the science of psychology is founded are the sensory stimuli that act upon organisms and the molar behavior of the organisms. Psychology has established many correlations between the impingement of given stimuli on an organism and the organism's consequent behavior—stimulus-response relationships. Meanwhile, between stimulus and response certain events occur within the organism. These events, it is clear, have their character determined partly by the nature of the impinging stimulus and partly by various organic states.

The organic states and events correspond to one class of what Tolman (**35**, **36**) has called *intervening variables*. MacCorquodale and Meehl (**16**) have recently pointed out that this expression has often been used interchangeably between constructs which are a summary of empirical relationships, and hypothetical constructs about "entities and processes not among the observed." Statements about the organic states that intervene between stimulus and response, inasmuch as these states are not directly observed, would be statements about constructs of the second type. Such constructs amount to imaginary pictures of the intervening processes. These pictures come as close as possible to the actual events if they are built up on the basis of whatever positive knowledge we already have of the functions of the nervous system.

Recent behavior theorists, led by Tolman (**35**), Hull (**15**), Skinner (**30**), and Spence (**32**), have assumed that present knowledge of the nervous system is not sufficient to yield a worthwhile picture of the intervening organic processes. As a consequence, their theorizing has generally (though not always) corresponded to MacCorquodale and Meehl's "empirical summary" type of construct. Nevertheless, that an approach to an understanding of the intervening organic events should be made whenever possible has been clearly recognized by Hull (**15**, pp. 18–19). The present paper purports to show that current neurophysiological knowledge is quite adequate to support a hypothesis regarding the organic events of the learning process. Moreover, the construction of this hypothesis seems to have the effect of clarifying a number of controversial issues.

Reinforcement As a Neural Process

It is a truism in psychology that the behavior of an organism in response to its environment is determined partly by maturation and partly by learning. When a knee-jerk results from a tap on a man's patellar tendon, the response is regarded as subserved by neural connections that maturation has established. On the other hand, such stimulus-response sequences as the salivation that follows the sound of a bell are established not by maturation but by learning, although, of course, maturation provides the basis upon which learning can occur. If it is presumed that the stimulus-response sequences set up by learning depend, just as matu-

[1] The writer is indebted to Dr. L. J. Reyna for some valuable suggestions.

rationally established sequences do, upon the development of functional neural connections, then the *process* of establishing such connections is the essence of what is called *reinforcement,* and an individual event that has the effect of initiating or strengthening a functional neural connection is *a reinforcement.*

As a matter of fact the neural basis of learning has been directly demonstrated by an experiment of Culler's (7) that has not received the attention it deserves. This investigator first of all conditioned some dogs to contract the right semitendinosus muscle in response to a tone. Then under complete anaesthesia the left cerebral cortex was exposed for direct stimulation. It was found that, so long as the semitendinosus muscle reacted to the tone, it reacted also to direct electric stimulation of a small spot not more than two square mm. in area on the anterior ectosylvian gyrus 20 to 25 mm. from the motor point for the semitendinosus muscle. When the tonal conditioned response was extinguished, so was the response to stimulation of the spot; and when the former response was reinstated, so was the latter. No other surface area with these properties was found. Dogs tested before conditioning showed no evidence of semitendinosus contraction on stimulation of the anterior ectosylvian area. Thus the reinforcing of the tone to the semitendinosus response is evidently correlated with certain neurones being linked to the neurones which subserve the semitendinosus response.

It is altogether reasonable to suppose that where learning involves the formation of conscious associations the process is basically similar. Suppose, for instance, that I learn that the word "window" means a window. Hearing the word spoken subsequently conjures up in me the image of a window. This implies that neurones responding to the word "window" have become so connected to the neurones subserving the visual image of a window, that stimulation of the former results in stimulation of the latter.

The Role in Learning of the Reduction of Primary Needs

Numerous observations have indicated how important a part is sometimes played by the reduction of what are commonly called needs in the occurrence of learning. For example: Gantt (9) reported that laboratory studies had shown that an increase in the amount of food used during conditioning was correlated with increased strength of the conditioned response. Skinner (30, pp. 360–361) found that rewarding thirsty rats with water immediately after lever-depressing activity resulted in an augmentation of the tendency for this activity to occur in the given situation. Grindley (11) demonstrated that whereas the pecking responses of chicks to the sight of grains of rice were strengthened if they were allowed to eat the rice, the responses progressively weakened (after an initial improvement) if the eating was prevented by the interposition of a sheet of glass over the grains. An experiment by Wolfe and Kaplan (39) showed that chickens that received a large piece of food at each trial developed superior performance in speed of running down a straightaway track and in negotiating a T-maze, in comparison with chickens receiving a piece of food a quarter as large at each trial. Finally, as Hull (15, p. 79) has pointed out, in situations in which a noxious stimulus is acting on an organism, out of the multitude of molar acts that may comprise the organism's responses, the one that is learned is the one that is followed by a removal of the noxious stimulus and

a termination of the need created by that stimulus.

DEFINITION OF NEED

At this point it is necessary to clarify what the essential characteristic of a need is. What is it that the various states which are called *states of need* have in common that enables them to be classed together under a common heading? As a starting-point we may list some of the more striking states of need: the need for oxygen, the need for water (thirst), the need for food (hunger), the need to excrete, the need for sexual satisfaction, the need for rest after exertion, and the need to be relieved from certain sensory stimulations, such as those associated with tissue damage or disturbance (*e.g.*, pain).

A need is, of course, something predicated of an *organism;* and in common usage the word derives its application from human feelings involving "wanting" or "unsatisfaction." Probably because allusion to extra-organismal referents aids communication, it has become customary when speaking of each different need-feeling to refer to it in terms of whatever agent has come to be associated with reduction of the particular need. Hence we have food-need, need to defaecate, and so forth. It is certainly convenient to have such terms of reference, but in order to determine what is common to all needs we have to know what goes on *within the organism* in the case of each need.

Now by no means everything is known about the physiological states underlying the various needs, but there is no doubt that they manifest great diversity. The need for food is correlated with the occurrence of rhythmic gastric contractions among other things (**5**, pp. 289–298); the need for water with dryness of the mouth and throat as a result of reduced salivary secretion owing to diminished body fluid (**5**, pp. 299–333); the need for physical rest with metabolic changes in muscles, and notably the accumulation of lactic acid (**38**); the need to micturate with fullness of the bladder producing tension on its walls; and the need to copulate with (*inter alia*) increases in the amount of circulating sex hormone (**1**, **2**, **21**, **22**, **28**).

If the physiological correlates of the various needs are compared, there seems to be no characteristic that is common to all of them—that is, if the physiological correlates of each need are surveyed in isolation from their consequents. Examination of their consequents shows that they *do* have something in common: they are one and all antecedents of neuro-effector rseponses. In a number of instances it has been shown that there is a correlation between the magnitude of some physiological aspect of the need-state and the amount of motor activity displayed by the organism. This has been found in relation to the food-need by Skinner (**30**, pp. 396–397) and Richter (**26**); and in relation to sexual needs by Richter (**26**), and Wang, Richter and Guttmacher (**37**). The studies of Beach (**1**, **2**), Miller, Hubert and Hamilton (**21**) and Shapiro (**28**) indicate the same sort of correlation.[2]

That needs produce effector responses through neural excitation was directly demonstrated by Pack (**23**) in the case of physiological lack of water. He showed that if the oral mucous membrane of a water-deprived animal is kept moist by the injection of pilocarpine, the animal does not drink water placed before it. On the other hand, as a number of facts mustered by Cannon (**5**, pp. 299–333) show, drying of the mouth results in drinking even when there is no physiological lack of water. Evidently, the motor activity associated with lack of water does not

[2] For a review of recent work see Beach (**3**).

Antecedent conditions, *e.g.*, drying of mouth (need state)

↓

Stimulation of sensory endorgans

↓

Central neural excitation

A physiological need-state produces central neural excitation through neural agencies. Need-reduction can subserve reinforcement only if central neural excitation is a consequent of the need state.

occur unless the lack of water results in excitation of the central nervous system as a consequent of the stimulation of those oral afferent nerves that are stimulable by dryness (see figure).

If being antecedents of neuro-effector responses is the only thing need-conditions have in common, by what are they to be distinguished from "ordinary" sensory stimuli which are also antecedents of neuro-effector responses? The answer seems to be that no absolute difference exists; and if the stimulating effects of "primary needs" stand out in any way from those of other stimuli, it is only in that they tend more often to be *strong* as measured by strength and extent of effector responses. Every sensory stimulus, therefore, has the essential characteristic of a need.[3]

[3] It is worth noting, however, that the converse is not true, for not every need condition functions as a *sensory* stimulus. For instance, when, as in the experiments mentioned above, an increase in circulating sex hormone is correlated with increased molar activity, the increase in activity seems to be due to the hormone lowering the resistance of certain nervous pathways, without any increase in the number of *afferent* impulses taking place (as far as can be judged). This was well shown by an experiment by Beach (2), who compared the behavior of testosterone-injected with that of non-testosterone-injected male rats in the presence of a variety of animals of different sexual "stimulus value" and in each case noted greater activity in the injected animals.

A New Conception of Drive

Whatever the mechanisms involved, need conditions, being antecedents of neuro-effector responses, are stimulus conditions to these responses. But between the stimulus conditions and the effector responses ***there intervenes excitation of neurones in the central nervous system;***[4] and to this excitation the term ***drive*** may usefully be applied.[5]

The conception of drive employed by Miller and Dollard (20) has much in common with that arrived at here. For them, however, the word refers, not to the neural excitation, but to the antecedent sensory stimulus. They state (20, p. 18):

> "A drive is a strong stimulus that impels action. Any stimulus can become a drive if it is made strong enough. The stronger the stimulus, the more drive function it possesses. The faint murmur of distant music has but little primary drive function; the infernal blare of the neighbor's radio has considerably more."

A weakness of their formulation is that it seems to imply that only excitations that result in *overt* activity qualify as drive. From the point of view of the learning process, as will be seen later, it is operationally more useful to say that all central excitation is drive, and when it reaches a certain intensity and if other conditions are present that enable the excitation to reach and excite

[4] "Central nervous system" is used in the present context not in opposition to "autonomic nervous system," but in Sherrington's sense, as that part of the nervous system in which "afferent paths from receptor-organs become connected with the efferent paths of effector-organs, not only those adjacent to their own receptors, but, through '*internuncial*' (J. Hunter, 1778) paths, with efferent paths to effector-organs remote" (**29**, pp. 312–313).

[5] This use of the word "drive" has affinities with the popular use which has its origin in the *feelings* people have of an impulse to action. Such feelings have correlates of strong central neural excitation.

effector neurones, overt motor activity results.

This conception of drive has the important consequence that since a reinforcement is an event involving the setting up of neural connections, drive-reduction, understood as reduction of central neural excitation, presumably plays a part in the learning process, particularly in view of the quantitative relationship that has been observed between learning and need-reduction (of which drive-reduction is a consequent).[6] (See figure.)

LEARNING WITHOUT THE REDUCTION OF PRIMARY NEEDS

A considerable number of observations have shown that for learning to occur it is not essential for the responses of the organism to include the reduction of a drive that is the consequent of one of the "primary" needs. Brogden (4) presented to each of four unselected dogs a flash of light and the sound of a bell simultaneously for 20 trials on each of 10 successive days. Then the light alone was followed by an electric shock to the left foreleg for 20 trials daily until the dog lifted its leg to the light flash and avoided the shock 100 per cent of the time. The next day the bell was given alone, and all dogs withdrew their left forelegs on 2 to 8 of 20 presentations. Of control animals which had never been presented with the bell-light combination and which were given light-shock training, only one ever gave any response to the bell subsequently.

That the bell was able to evoke the overt response reinforced to the light indicates that the bell stimulus must have produced some internal response in common with the responses produced by the light stimulus; and the mere fact of their repeated previous temporal contiguity, in the absence of the reduction of any "primary" need, is all there is to explain this. Similar experiments with similar results have been reported by Pavlov (24, pp. 33–35)—who termed the responses so established "secondary conditioned reflexes,"—and by Eccher and Culler (8).

The connecting of stimuli to new responses in the absence of the reduction of a primary need is also demonstrated by the experiments of Cason (6) who reinforced pupillary reactions to auditory and electrical stimuli, and of Hudgins (13) who reinforced the same reactions to auditory stimuli; by those of Menzies (18) and Gottschalk (10), who reinforced vasomotor responses to auditory stimuli; and by those of Schlosberg (27) who reinforced the knee-jerk to auditory and tactile stimuli.

As a final example may be mentioned the concatenating of experienced events in temporal series that gives human memory its continuity. If A and B are two past events such that A was immediately followed by B, *one* way of recalling the image of B to the experiencer is to present a stimulus that evokes an image of A. The image of B may be recalled by this means even though there is no evidence of any other special link between A and B. Suppose, for example, that an hour ago, as I stopped my car, it began to rain. If now, at the present moment, some combination of stimuli evokes in me an image of the stopping of my car, I am quite likely also to remember the starting of the rain. This surely means that a connection now exists between the neurones that responded to *this* car-stopping and those that responded to *this* starting-of-rain, so that reactivation of the former could result in reactivation of the latter. There is no reason to think that in the establishment of a connection like this, which happens to have correlates in the con-

[6] A detailed neurophysiological hypothesis of reinforcement is presented elsewhere (40).

sciousness of the organism concerned, the underlying neural process differs in any essential way from that which underlies the formation of a simple conditioned reflex. In such learning as in the above example there is no evidence of the reduction of a drive that is associated with a primary need.

Now it is certainly undesirable to postulate different mechanisms for the learning that occurs in association with primary need-reduction and that which occurs without it; and if reduction of central neural excitation plays a part in one it should also play a part in the other. In all the situations described above in which learning occurred in the absence of reduction of a primary need, it is easy to see that central neural excitation was produced by such stimuli as were present. This excitation diminished when any of the antecedent stimuli ceased to act. Therefore, the learning observed was correlated with the reduction of some measure of central neural excitation or drive. It would appear that a large variety of excitations of the central nervous system, may, in association with their potentiality of being reduced, subserve the formation or strengthening of new neural connections.

That the most powerful reinforcements generally occur in contiguity with the reduction of drives related to the primary needs seems to be due simply to this, that the primary needs lead to the strongest of excitations in the central nervous system and these necessarily have the strongest drive-reduction potential. Yet very strong states of excitation certainly do occur even when there is no evidence of the reduction of a significant measure of any primary need. The reason is that, through the learning process, practically any stimulus may be enabled to evoke central excitations of a strength and of a persistence comparable with the drives created by primary needs. A great deal of profoundly important human and animal learning is related to the reduction of such excitations (**17**, **19**, **20**, **40**).

Some Consequences of the Present Theory

Certain consequences for behavior theory that emerge from the present formulation will be briefly mentioned:

(1) The basic difference pointed out by Spence (**31**) between contiguity theorists like Guthrie (**12**) and reinforcement theorists like Hull (**15**) appears to be resolved in favor of the reinforcement theorists to the extent that, from a neurophysiological standpoint, drive-reduction can be predicated of the organism in all situations in which learning occurs just because all sensory stimuli are antecedents of what has been called drive, and drive is reduced when stimulation ceases. On the other hand, when drive is regarded in this way, it is superfluous to postulate as a special class of stimuli, drive stimuli, such as Hull's S_d unless to distinguish stimuli of high intensity. Meanwhile a rationale is provided for Thorndike's idea (**34**) that "connections" are strengthened slightly by merely occurring, and much more if there is also reward; for clearly the presence of what is called reward simply implies *greater* drive-reduction.

(2) Hull's conception of primary and secondary reinforcement as *separate* principles, the first to account for learning in association with the reduction of a primary need, and the second to account for learning in the absence of such reduction, turns out to be unnecessary, as Hull himself suspected would be the case (**15**, pp. 99–100).

(3) To explain delayed reward learning, postulation of the evocation of fragmentary goal reactions along the pathway to a goal (Hull's goal-gradient hypothesis, **14**), and postulation of sec-

ondary reinforcement (Spence, **33**), are alike unnecessary. It is possible to account for such learning by assuming that reduction of a powerful drive has a reinforcing effect upon synapses, and that the effect is greater the more recently the synapses have been in action. This has been discussed in some detail in another connection (Wolpe, **40**); and the writer intends to deal with the subject more fully in a forthcoming article.

(4) It is believed that the present paper goes some way towards answering the questions posed by Postman (**25**) in his review article on the law of effect.

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THE STIMULUS FIELD IN SOCIAL PSYCHOLOGY

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In the literature on social psychology, stimulus situations are sometimes described by such adjectives as ambiguous or unambiguous, structured or unstructured, well-structured or poorly-structured. There is considerable confusion concerning the meaning and applicability of these and related terms, and the relationships among them. To cite one example: in a section entitled "Frames of reference in relation to structured and unstructured stimulus situations," Sherif and Cantril (9, p. 43) seem to equate the term *structured* with the term *well-structured,* the term *unstructured* with *poorly-structured,* and the concept of structure with the concept of ambiguity; but Chapman and Volkmann (2, p. 98) are against this use of the terms "structured and unstructured stimulus situations," advocating the application of the concept of structuredness, not to stimulus situations, but to the subjective frames of reference which they produce, and preferring to speak, as does McGregor (6), of "ambiguous and unambiguous stimulus situations." Since the concepts of structure and ambiguity have implications for theories of perception and since differences among various explanations of perceptual and social phenomena may be partially accounted for by the fact that the aforementioned terms are employed with different shades of meaning, it seems worthwhile to aim at precise definitions in order to provide a common universe of discourse. It is not intended in this report to furnish such definitions (nor do we think this a task to be tackled singlehandedly), but rather, to stimulate efforts in this direction.

The term *unstructured,* although frequently employed, is generally not carefully defined. Literally it means nonstructured or structureless. But what stimulus object has no structure? And if it is structureless, what distinguishes it from its background, and how does one perceive it?

Gestalt psychologists have employed the word *structure* in contrast to *and-summations* of elements where the latter refers to "that kind of togetherness from which any one or more units may be removed without any effect either on the ones remaining or the ones removed" (3, p. 25).

Perhaps the term *unstructured* is intended to describe such and-summative situations; or perhaps those who use it assume implicitly that the word means "possessing a low degree of structural clarity." In any event, the use of the term is a source of confusion unless a contextual definition is given.

The term *unstructured* has been applied to such objects as inkblots (*e.g.,* 8, p. 432). Certainly the nature of an inkblot may be altered if its parts are altered; therefore, strictly speaking, it is not an and-summation. A more appropriate descriptive term is *ambiguous* where ambiguity refers to the fact that the situation does not possess properties which impose only one clear, persistent organization, but that it is compatible with various figure-ground relationships.

The term *ambiguous* has been used as synonymous with *unstructured* and *poorly structured.* If we accept the above proposed definition, then an ambiguous situation is not one which has no structure, or which is necessarily of an and-summative type, but one which

allows various structurizations. Some may be attained and maintained more or less readily than others; some or all may be relatively poor configurations. It may happen, however, that some of the possible structurizations are clear, quite dominant, and good configurations. Consider Boring's "beauty-hag" picture. If the subject sees the young woman, it may be for him a very strong structure, difficult to alter. Once he does perceive the hag, she may become so dominant that he encounters difficulty in restoring his former view of the picture. Each of the two organizations is strong and compelling. Thus ambiguity is not equivalent to a low degree of structural clarity. As a matter of fact, it does not at all refer to the strength of structurization, but rather, to a range of possible structurizations.

Ambiguity is sometimes regarded as akin to neutrality. This meaning is somewhat confusing since an ambiguous situation does not permit just any reorganization of its parts. Moreover, some structurizations may be more readily attained and maintained, so that the stimulus field, even though ambiguous, is not neutral but plays an active role in determining what structurization is achieved. In one of our experimental studies (**5**), for example, we found that when confronted with a drawing sufficiently ambiguous to elicit such diversified replies as "face, girl, eye, eyebrow, lips, tongue, hair, shoulder, finger, fair lady, letters of the alphabet, numerals, circles, lines, scribbling, Veronica Lake, intestines, music notes" (**5**, pp. 259–260), not one of our subjects agreed that he saw in it a battleship or a bottle (**5**, pp. 270–271). The conclusion drawn from this study was that whether or not a subject agreed with the socially offered response depended to some extent upon whether the objective material gave the possibility of support of this description, whether it possessed some leading characteristics or the possibility of being reorganized in line with it. The drawing, although ambiguous, lent itself to some figure-ground relationships, but was incompatible with others.

It may not be out of place to refer here to what has been accepted as a truism by some psychologists, namely, that the greater the ambiguity or the less definite the structure of the stimulus object, the greater tends to be the influence of personal and social factors (as opposed to autochthonous factors) in determining the percept (*cf.* **1**, p. 97; **7**, p. 351; **9**, p. 45). Indeed, the present writer accepted this viewpoint in an earlier publication (**5**, p. 257). But it now seems to us that this "truism" requires critical evaluation.

To begin with, are there not conditions under which personal and social factors may make one better aware of the nature of external stimulation? Are not the various educational campaigns aimed at combating propaganda, prejudices, and superstitions, based on the thesis that social pressures may open one's eyes to reality? Why then has experimentation designed to study personal and social influences on perception devoted itself, almost without exception, to the blinding influences of such forces rather than to their possible eye-opening effects?

Secondly, as was indicated in foregoing paragraphs, it may be a false dichotomy to distinguish between ambiguous stimuli and well-structured stimuli, as if ambiguity necessarily precluded a high degree of structural clarity.

Finally, the combined results of our experimental studies on social perception (some still unpublished) do not reveal a direct relationship between the reported percepts and the relative degree of structuredness or of ambiguity of the various geographical stimulus objects. Thus, in highly ambiguous draw-

ings, even when social pressures favoring conformity were introduced, it was found that not every proffered response was accepted as a frame of reference to be used in organizing the material. Even when clear-cut stimulus material was presented, under certain conditions (*e.g.*, under the influence of speed, competition, or hunger) subjects could be made to give bizarre, contaminated responses which were never reported by control group subjects. While it was our impression that in some of these cases a subject actually saw the dominant features of the objects although he failed to report them, it nevertheless seemed clear from the manner in which some subjects gave their responses and their reactions when they were later shown these objects that they had previously failed to see characteristics or organizations usually reported by control group subjects, and had actually believed that they were seeing what they had reported. In drawings graduated for structure it was found that under certain conditions, as the clarity of the stimulus object increased, there was not a noticeable increase in the role played by prehensible features of the drawings or a decrease in the role played by personal and social factors in determining the percept (see, for example, 4).

In order to understand the relation between stimulus and percept it seems necessary to distinguish between the geographical stimulus and the actual stimulus field which the subject uses in forming his percept. Social influences and motivational factors are just as much stimuli for perception as is the geographical stimulus object presented for judgment or observation, and they may become very dominant parts of the subject's behavioral stimulus field. It is not enough, therefore, to be concerned with the degree of structural clarity or ambiguity of the geographical stimulus; it is important to inquire into the extent of ambiguity—or permissiveness—of the entire perceptual situation, and the degrees of structural clarity of the possible structurizations. These may be influenced by various factors in the perceptual field, such as physiological and structural conditions of the observer's perceptual processes, *e.g.*, brain-damaged and toxic individuals may be unable to comprehend certain forms and structures.

What is needed is a functional description of the life-space of the subject at the time of perception which takes into account the structural character of the behavioral stimulus field, and the role and function in this field of the assigned stimulus object. It seems to us that regardless of the structural nature of the material given for observation, a subject will tend to be guided or not guided by its autochthonous factors according to whether it plays a central or peripheral role in his life-space at the time of perception.

To summarize: It seems to us that it is necessary, for proper conceptualization of social perception, to examine the meanings of such terms as unstructured, poorly structured, ambiguous, as well as structured, well-structured, clear-cut, and unambiguous, and to aim at precise definitions. It is necessary to reconsider some prevailing conceptions concerning the relationship between the structural nature of the stimulus object and the resulting percept. Finally, in order to advance understanding of this relationship, inquiry into the nature of structurizations should not be limited to the geographical stimulus object but should be extended to the behavioral stimulus field (which may or may not include the object given for judgment), and, indeed, to the entire perceptual field, *i.e.*, to the perceiver's momentary life-space.

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THEORETIC KNOWLEDGE AND HYPOTHESIS [1]

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There is one problem in which all psychologists and all philosophers are interested—the problem of knowledge. If they are not, they ought to be. It is true that sometimes one meets either a scientist or a philosopher who thinks and says that he is not interested in the problem. I have had a philosopher say to me, "I am interested in metaphysics not epistemology. First we must find out what reality *is*. Knowledge is just a part of it." Similarly, I have had a scientist say to me, "Why raise problems about knowledge? I look and see. The facts speak for themselves." This scientist was not a psychologist, and I hope that psychologists are not so naive either about seeing, or about how loquacious facts are.

It is sometimes said that too much emphasis on the criticism of method is a sign of decadence in any field of study. Perhaps it is; but on the other hand, too little interest may be a sign of naiveté. The person who takes methodology or the problem of knowledge for granted is at the mercy of various unconscious prejudices and assumptions. These unrecognized and therefore uncriticized assumptions will continually creep in to vitiate his findings, and to set him at sword's points with those of his colleagues who have acquired different assumptions.

I have nothing against assumptions as long as they are not unrecognized and uncriticized. In fact, I am going to start from an assumption which I am explicitly calling to your attention. I shall present it to you here uncriticized, but that is because of the limitations of time and of your endurance. I have criticized it elsewhere. The assumption is that the most reliable way to gain generalized knowledge about the world around us—how it is constructed and what is in it—is by the use of the scientific method.

Note that I say *generalized* knowledge. That means theoretic knowledge. And note also that the theoretic knowledge with which my assumption is concerned is the knowledge of the world of experience. The application of the scientific method in the way indicated gives rise to the empirical sciences, or, if you wish, the natural sciences.

I suppose that all of you will agree with me in my basic assumption; and certainly the large majority will agree also that psychology is one of the empirical sciences. Thus the method of gaining reliable generalized knowledge in psychology is by the use of the scientific method. Most of you are acquainted with the role of hypotheses in natural science. All natural laws are, in the last analysis, generalizations of great scope, and these generalizations are hypothetical in nature.

Science does deal with facts, but only as raw material. Facts are useful in so far as they yield laws, theories, hypotheses. They do not speak for themselves. No fact ever told an investigator what law it embodied. The investigator has to dig the generalization out of the accumulated mass of facts.

The interrelationship between fact and theory itself constitutes a problem. The ascertainment of fact is itself not independent of theory. Facts are not

[1] Read as the presidential address at the Forty-first Annual Meeting of the Southern Society for Philosophy and Psychology, April 15, 1949.

what is ultimately and irreducibly given in experience. Facts are what we are aware of in perceptual experience,—but that is a different matter. It was long ago pointed out by psychology that concrete perception always includes a reference to past experience. Perception of fact is an interpretation of what is immediately and irreducibly given in the activity of the senses and in imagery in terms of past experiences and future possibilities of action. Facts, in other words, always include a conceptual element. Facts mean something, both in reference to past experience and to future action, and meaning is conceptual.

We may call whatever is ultimately and irreducibly given in experience, perceptual intuition, *i.e.*, that of which we are immediately aware in perception. It is true that in concrete experience we are aware of facts, and that the intuited elements are analyzed from the concrete facts; but this is because we are all more or less adult human beings with inescapable references to past experience and future possibilities of action. We cannot free ourselves from the past and the future except by the processes of analysis and abstraction.

The colors, shapes, and sounds in our experience are intuited, but these intuitions are never the whole of concrete adult experience. The concrete object of our perception is a tree or a telephone pole or juke box. These are perceptions of fact, and facts are always interpretations of intuited content in terms of concepts. "Tree," "telephone pole," "juke box": these words name concepts. I remind you, however, that I am not here making inexpert and speculative pronouncements about genetic psychology. I am engaged in epistemological analysis.

These considerations, however, are taking us afield. My subject is not the nature of fact but the nature of theory. It is true that fact is not independent of theory, but neither is it the same as theory. Theory is general. Facts are particular. That means that a fact has a special locus in space and time, and each separate fact has a different locus. Theory is not tied down to a special locus. Theory is general. This means that it applies to many facts in many different loci of space and time. Facts may be perceived. Theories, on the other hand, are conceptual.

I realize that the word "theory" is not always used in the way in which I am defining it. A newspaper editorial, in condemning a view contrary to its own, will sometimes say, "That is only theory; it won't work." Such uses of the word "theory" are not very precise. I do not know exactly what the editorial writer means. The expression is probably his way of saying that, in his opinion, his opponent's views are hot air, empty and distasteful to all right-thinking persons.[2]

In this paper I shall use the words "theory" and "theoretic" to refer to knowledge that is general in nature. General principles are theoretic, and a body of interrelated general principles is an elaborated theory.

We are now ready for the major thesis of this paper. It is as follows: all theoretic knowledge is hypothetical.

As soon as I make this statement, the status of mathematical and logical principles may occur to you. These are indisputably theoretic, and they are usually regarded as certain. Their certainty, however, is only ***relative to their premises***. If the postulates of a mathematical or a logical system are true or correct, then the theorems are certain. The foregoing statement is certain, but note that it is hypothetical.

These statements, however, do not represent the kind of theoretic knowl-

[2] As Sir Arthur Eddington remarks: "'Right-thinking person' is, of course, a modest way of referring to oneself" (**1**, p. 2).

edge nor the kind of hypothesis into which I want to inquire. The certainty here arises because the mathematical and logical principles are not about anything. They may be *applicable* to experience, but they are not *about* experience. Mathematical and logical principles are certain *a priori,* and that is because they are analytical and do not carry with them any statement of their applicability.

When I say that all theoretic knowledge is hypothetical, what I want to call to your specific attention is theoretic knowledge about the world we live in, generalizations about empirical matters, statements whose meaning lies in their application to experience.

The type of hypothesis to which I am referring is the explanatory hypothesis of natural science. Such a hypothesis, when it fully and explicitly performs its explanatory function, is stated in the form of a proposition in formal logic—the "if-then" proposition, where the "if" clause is called the antecedent and the "then" clause the consequent.

It is customary for writers on science to distinguish between a scientific hypothesis and a scientific law. I shall nevertheless maintain that formally there is no essential difference. If we wish, we can refuse to call any principle a law unless it is wide in scope and has received a high degree of verification. It may nevertheless remain true that the knowledge which this law expresses is hypothetical. I think that the day is past when scientists can say with Karl Pearson:

> "Scientific law is valid for all normal human beings, and is *unchangeable* so long as their perceptive faculties remain at their present stage of development" (5, p. 104).[3]

I hope that the day is past when scientists are *disposed* to say this, especially when they place emphasis on the word "unchangeable."

The terms "law," "theory," "hypothesis" can all be used in a narrower or a broader sense. In the narrower usage the distinction between them depends on a difference in the degree to which they are based on evidence and have been confirmed by evidence. Thus we can call a generalization based on little evidence and with little confirmation a guess or speculation; with more we call it a hypothesis; with still more, a theory; and with most evidence and confirmation, we call it a law. There is nothing very precise about this contrast of terms. The usage is mostly traditional. We still talk about the law of gravitation, but about the theory of relativity.

What I am proposing is that in the broader, more fundamental use of the terms, all generalizations are theoretic and theory is hypothetical. I am not talking merely about the use of words. I am talking about the nature of theoretic knowledge, no matter what it may be called: whether it is categorical and yields certainty or whether it is always hypothetical and yields only probability.

Let us return to the laws of science. These are inductive; that is, a generalization is stated on the ground of experience with particulars. Of course these laws are usually stated categorically, but this is merely a linguistic convenience. The knowledge that they convey may not be categorical. The kind of knowledge that they do convey can be found in a consideration of their genesis and the relation they have to the particulars. Generalizations do not

[3] Italics mine. Pearson emphasizes that scientific law is relative to human beings, but he used the word "hypothesis" only for a "working hypothesis." In his terminology a generalization is a hypothesis when it has not been or is being "put to the test of experience." It is a law when it has passed the test. See p. 120.

just grow on particulars and wait for us to come along and pick them off.

The important point is that generalizations are always intended to be explanatory of the particulars. We say that if such and such a generalization holds, then this and that particular will follow. To take a concrete example: "If all policemen have big feet, then it is not surprising that Willie, the pride of the finest, wears number 12 shoes."

It would be better to cite more important generalizations. In 1919 the Royal Astronomical Society sent expeditions to Brazil and to West Africa where a total eclipse of the sun was predicted to take place. The expeditions were to observe especially the positions of stars close to the sun, because if the general theory of relativity holds, then the apparent position of stars close to the sun should be displaced outward approximately 1.7 seconds of an arc. No one had ever noticed this phenomenon, but now when it was looked for, the phenomenon did make its appearance. The stars were observed to be displaced approximately the amount predicted, subject to inaccuracy of observation and measurement (2, pp. 152–155). In 1922, with instruments of greater precision, the displacement was further verified (3, p. 17). These were crucial observations in establishing the general theory of relativity.

Note that I have been careful to retain the hypothetical form of the statement of the principle. "If the systematic body of generalizations making up the theory of relativity holds, then certain events will take place." Thus on the ground of accepting the general theory, the events, when they do take place, are explained. When the events have never been observed before their prediction, the explanatory power of the hypothesis is spectacularly revealed upon their observation. Cases in point are the first observation of the planet Neptune and the case I have already described, the displacement of stars close to the sun. Nevertheless, the principle is the same when the hypothesis explains something previously observed, such as the advance of the perihelion of Mercury (2, p. 150; or 3, p. 22) or the occurrence of an ordinary eclipse of the sun or moon.

When I say that scientific hypotheses explain particular occurrences, I am not using the word "explain" in a metaphysical sense. That is, the hypothesis is not the "reason why" the particular event takes place. Perhaps the assumption that the event takes place because it is commanded to do so by the generalization gave rise to the term "natural law" (6, p. 245). No scientist today, however, would hold this view, and the use of the term "law" is understood to be derived from a metaphor. Scientific explanation of particulars means that the occurrences of many different kinds of particulars can be related to each other in an intelligible manner by means of generalizations under which all can be subsumed.

A scientific hypothesis or theory is a generalization of wide scope which expresses the correlations of many particular events. It can be assumed to be true only to the degree to which it accomplishes this task. It is not known *a priori;* it is not given in any immediate knowledge. We say it is elicited from the particulars themselves.

This process can be described briefly as follows. Particular experiences are perceived. As soon as we start thinking about them—in fact, perhaps in the very act of perception itself—they are subjected to analysis, that is, they are broken up by thought into parts. If any part or any collocation of parts is regarded by itself or without reference to other parts, the process of abstraction has taken place. Abstraction means thinking of something outside of the

concrete context where we find it. Generalization consists in noting that we can abstract the same characteristics or conditions from more than one particular instance. Thus a generalization applies to or covers several particular instances.

Such in brief is the genesis of a scientific hypothesis, theory, or law. Let me summarize as far as I have gone. A scientific theory says, "If generalization G is true, then particulars a, b, c . . . follow." The latter are subsumed under the generalization and thus explained by it. If we accept the generalization, the particulars of experience are rendered intelligible. This is the only way of rendering the particulars of experience intelligible—relating them to each other by means of generalizations under which they can be subsumed. Thus the general theory of relativity explains the displacement of stars near the sun and the progression of the perihelion of Mercury. Newtonian mechanics and gravitation do not explain these phenomena. This means that they cannot be subsumed as special cases under the generalizations of the Newtonian theory.

Now to go on: I am proposing that all inductive knowledge, whether it is to be found in the body of natural science or in philosophy or elsewhere, is of this same nature. We have no other way of making valid generalizations from experience than by analysis and abstraction from our perceptions. There is no other way of rendering anything in experience intelligible but by relating it to other things through the generalizations thus obtained. By pure reasoning you can arrive at valid conclusions, but you cannot show that these conclusions apply to anything in experience. Thus all empirical theory (and by that I mean all theory about anything except itself) is made up of generalizations, the validity and the truth of which lie in their explanatory power and effectiveness. All empirical theoretic knowledge is hypothetical and finds its justification in its verification.

Now let us go back again to the verification of scientific hypotheses and consider them from a strictly logical point of view. The form is: if generalization G is true, then particulars a, b, c . . . will happen. Suppose that we observe a, b, and c. Does that prove G in any strict sense of the word "proof"? The answer is simply *no*. The general theory of relativity predicts that stars near the sun will be seen to be displaced outward 1.7 seconds of an arc. They are so seen. That does not prove the theory or even contribute to its proof. The theory predicts that the perihelion of Mercury will advance approximately 43 seconds of an arc per century more than predicted by the Newtonian theory. It is seen to advance at this slightly more rapid rate. Does that prove the theory? Again, of course, the answer is *no*, not in the strict sense of the word "proof."

To suppose that a hypothesis is proved by the truth of the consequences is to perform the simple fallacy of affirmation of the consequent. This is *always* a fallacy. Not all the consequences in the world can prove the hypothesis. The observation of consequences does contribute toward the verification of hypotheses, but verification is not proof. The observation of consequences is usually held to increase the probability of the truth of the generalization, though there is a dispute between adherents of different theories of probability as to how this can be.

What does it mean then to talk about the "truth" of a hypothesis? A scientific hypothesis is justified to the degree it is verified. Its verification lies in its explanatory power, and explanation means the degree to which different particulars of experience can be

interrelated and thus be shown to be intelligible by means of subsuming them under suitable generalizations. Theoretic truth then is defined in terms of rendering experience intelligible.

I suggest that this is the full meaning of empirical theoretic truth. Perhaps practical truth and perceptual truth are different matters and differently to be explained. That question is not at issue now. I am giving a theory of theoretic truth, and when I say that my theory of truth is true, I aim to be consistent. I mean that it is a hypothesis which, so far as I can see, has great explanatory power.

I do not see how psychologists or philosophers can claim to be exempt from the consequences of this hypothesis. Mathematicians and logicians are exempt, for their theories are not about empirical matters. Both psychologists and philosophers, however, claim most of the time to be talking about something in experience. In so far as they are concerned with experience, they are not exempt. Their theories are hypotheses that are justified only by their explanatory power.

At the present time there is a good deal of feuding between different schools of psychological thought. It is taken for granted that philosophers are always feuding. I think that this is not only regrettable, but if there is anything in the thesis of this paper, it is theoretically unjustified and perhaps even a little absurd. No one has any theoretic truth that is more than hypothesis. It does not seem probable from an examination of how hypotheses are formed that any one hypothesis can be established so firmly as to render all alternatives impossible.

Feuding among philosophers and psychologists—and especially the intellectual intolerance upon which it is based—is theoretically unjustified. This is not meant, however, to discourage lively controversy. When a new hypothesis is proposed it should be attacked and defended with vigor. Only in this way can it be thoroughly tested. Today we are apt to judge that Priestley's attack on the new combustion theory of Lavoisier was not well taken. Perhaps Priestley held to the phlogiston theory too long and against too much evidence; but perhaps his attacks on the combustion theory helped to establish it by forcing its defenders to subject it to thorough testing.

On the other hand it is good that new hypotheses should not be accepted until all conceivable attempts to overthrow them have been made. It is also good that the supporters of a new hypothesis should hold on to it and push it, so that it will not get lost in the shuffle, or even worse than that, get squelched by authority.

There should be no such thing as scientific authority, although unfortunately there is. In the 1880s C. A. McMunn made a contribution to the understanding of the way in which the transfer of oxygen from hemoglobin to the tissues takes place. After a little controversy, he was put down by the bio-chemist Hoppe-Seyler, who twenty years earlier had made the very important discovery of the role of hemoglobin in carrying oxygen from the lungs to the tissues. Hoppe-Seyler printed a note alongside McMunn's last paper saying that he considered further discussion superfluous. McMunn apparently subsided; at any rate discussion was dropped, and it was not discovered that he was correct and Hoppe-Seyler incorrect until forty years later, in the 1920s (**4**, p. 170). It might have saved forty years if McMunn had been a little more tenacious of his hypothesis.

All philosophers and all psychologists are engaged in the same kind of task in so far as they are all trying to understand something. Understanding is

achieved by the formulation of generalizations and the construction of theory. Theoretic knowledge is hypothetical, and is true in so far as the hypothesis does what it is set up to do. There is no justification whatever for one person to assume that his theories are absolutely true and those of his opponents are so false that only a stubborn ass could continue to hold them. *All* hypotheses must be tested in every conceivable way. Let the opponents suggest and work up tests. Unless they have something constructive of this kind to say, let them keep silent and spend their energies not in quarreling but in developing their own hypothesis and welcoming the suggestions of tests made by their opponents. If the hypothesis will not stand the test, it is no good anyhow. A hypothesis cannot be established by refusing to test it, but only by testing it in every conceivable way.

We shall all get further toward our goal of understanding if we cooperate. The intellectual enterprise is a common and cooperative endeavor, and its furtherance should not be hindered by personal bickerings and unfriendliness that are utterly incompatible with its nature. The view that all theoretic knowledge is hypothetical gives no basis for dogmatism in either philosophy or science. The dogmatist is one who is absolutely certain of his knowledge.

If we are not to be dogmatists, then are we, *ipso facto,* skeptics? The thesis of this paper gives *no* comfort to skeptics either. The skeptic denies the possibility of theoretic knowledge. I do not deny it. On the other hand, I assert that we have it. We have a great deal of it. Furthermore, all understanding (in the literal epistemological sense of the word "understanding") is in terms of theoretical knowledge. And there are many things about this world that we do understand. The view that theoretic knowledge is hypothetical does not conjure away this understanding.

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CRITERION ANALYSIS—AN APPLICATION OF THE HYPOTHETICO-DEDUCTIVE METHOD TO FACTOR ANALYSIS

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I. Introduction

Among statistical methods now in common use in psychology, factor analysis presents certain interesting and paradoxical features. Although more and more psychologists, sociologists, and recently even physicists and chemists, have used factor analysis as their preferred statistical research tool, and although many experts, among whom we may mention Thurstone, Burt, Cattell, Guilford, and Vernon, have expressed their faith in its adequacy to solve some of our most pressing taxonomic problems, yet much criticism of this technique has been advanced by two schools of thought usually on opposite sides of the fence. Factor analysis has been criticized severely by those who, like Allport, Murray, and other adherents of the psychiatric, individualistic, idiopathic point of view believe its atomistic assumptions violate the holistic nature of human personality. It has equally suffered the onslaughts of professional statisticians who point out its formal deficiencies, and prefer the more rigorous methods of discriminant function analysis, analysis of variance, and regression equations. No other method in statistical psychology has suffered such a multiplicity of criticisms, and it behooves those of us who make use of it to look carefully at the various aspects of factor analysis which may be considered most vulnerable to such attacks.

Truman L. Kelley has pointed out that statistics has three main functions. "The first function of statistics is to be purely descriptive, and its second function is to enable analysis in harmony with hypothesis, and its third function to suggest by the force of its virgin data analyses not earlier thought of" (**9**, pp. 22, 23). While most statisticians would agree to the descriptive purposes of many statistical constants, there is less agreement regarding the other two functions mentioned by Kelley. "We may say that there are two occasions for resort to statistical procedures, the one dominated by a desire to prove a hypothesis, and the other by a desire to invent one. This has led to distinct schools of statisticians, both lying within the general field of scientific endeavor" (**9**, p. 12).

Reference to this dichotomy of functions and purposes within the field of statistics gives us one hint as to the reasons for the general statistical criticisms of factorial methods. Unlike analysis of variance and covariance, discriminant function analysis, or even the humble C.R. or *t* form of analysis, factor analysis does not in general attempt to prove or disprove a hypothesis; it does not set out to disprove any form of null hypothesis at some critical level of significance. Its function appears to be far more dominated by the desire to "invent" a theory, and in spite of Kelley's words this function of statistics is not generally recognized by statisticians as being truly within the purview of this particular branch of science.

Holzinger describes the nature of factor analysis thus: "Factor analysis is a branch of statistical theory concerned with the resolution of a set of descrip-

tive variables in terms of a small number of categories or factors. . . . The chief aim is . . . to attain scientific parsimony or economy of description" (7, p. 1). Similarly Kelley: "There is no search for timeless, spaceless, populationless truth in factor analysis; rather, it represents a simple, straightforward problem of description in several dimensions of a definite group functioning in definite manners, and he who assumes to read more remote verities into the factorial outcome is certainly doomed to disappointment" (10, p. 120). It is clear that these authors regard the task of factor analysis as essentially one of arriving at a convenient small set of fictitious variables which can be used to describe the interrelations of a larger set of (real) variables; these fictitious variables or factors are the hypotheses which the statistical method of factor analysis helps us to "invent." There can be very little doubt that factor analysis has played an important part in aiding in this task of "invention," and the writer does not wish to underrate the contribution to psychological theory which has been made in this way.

Yet when one looks over the list of factors found in the cognitive field (12), or in the field of conation and affection (1), one may be pardoned for concluding that most of these hypotheses which factor analysis has led modern writers to espouse are far from original, and may be found in writings free from any contamination with statistical procedures. If the true contribution of factor analysis has really been the "invention of hypotheses," then it would seem that other less laborious methods would frequently have given us hypotheses not essentially different from those emerging from this modern concourse of calculating machines. And if the rejoinder be made that hypotheses derived from careful statistical investigations of large-scale experimental materials are more valuable than hypotheses of the Schreibtisch-experiment type, the answer surely must be that the derivation of an hypothesis does not guarantee its value, but that what is required is some method of indisputable proof of the correctness of the conclusion. In other words, we are led back to the alternate function of statistics mentioned by Kelley, the proving of an hypothesis. Can factorial methods be used to prove, as well as to suggest, scientific hypotheses?

This question is taken up directly by Cattell. He points out that the psychologist, as a scientist, will want to find the set of factors which corresponds to a set of psychologically real influences because he is interested in understanding the psychological meaning of his predictions and because he is curious to gain truth for its own sake. "In that case he may (1) devise possible ways of overdetermining the analysis of the given correlation matrix so that only the one set of true factors will emerge, or (2) start from the opposite shore and propound, on psychological grounds alone, a hypothesis about what source traits are operative in the variables. Then he will see if these factors correspond to any of the possible mathematical factors found in the matrix" (1, p. 273). Cattell rejects the second, or hypothetico-deductive, type of method on two grounds. ". . . in the first place, personality study has so few other reliable avenues for arriving at, or even suspecting, the basic source traits, that hypotheses are likely to be erratic. In the second place, the mathematical solutions to any set of correlations are so numerous and varied that unless the hypothesis can be stated in very precise quantitative terms the 'proof' of it is easy—so easy as to be worthless" (1, p. 274). Cattell repeats his position in slightly different terms: ". . . we have

rejected one of the two major approaches normally approved by scientific method—namely, that of inventing a hypothesis about the particular factors expected and attempting to discover a factorization to match it—because in this field almost any hypothesis could be so 'confirmed.' Instead, we seek general guiding principles for the mathematical analysis itself which will lead to a unique solution" (1, p. 281).

It is the purpose of this paper to suggest a method of rotation of factor axes which will give a unique, invariant solution along the lines of the hypothetico-deductive method; in other words, we believe that Cattell dismisses too easily the most powerful instrument of scientific methodology so far devised, and advocates instead methods which we shall try to show to be in no way adequate substitutes for it. First, we shall turn to an examination of these "methods of overdetermining the analysis of the given correlation matrix," and to a review of the results which may be expected from the use of these methods; then we shall describe the principles on which the method of criterion analysis was devised, in an attempt to get over the difficulties pointed out by Cattell; the principles will be discussed by reference to a worked example to show the application of this new method to a concrete problem.

II. Principles of Factor Rotation

Cattell lists seven principles for determining the choice of factors: (1) Rotation to agree with clinical and general psychological findings; (2) Rotation to agree with factors from past factor analyses; (3) Rotation to put axes through the centre of clusters; (4) The principle of orthogonal additions; rotation to agree with successively established factors; (5) The principle of expected profiles; rotation to produce loading profiles congruent with general psychological expectation; (6) The principle of "simple structure" relative to the given correlation matrix; (7) The principle of proportional profiles or "simultaneous simple structure." These seven principles may in our discussion be reduced to two: rotation where there is an outside criterion, and rotation where reliance is placed exclusively on statistical properties of the correlation matrix.

When there is an outside criterion, there are many different ways of making use of the criterion; these all reduce ultimately to the most simple and direct—inclusion of the criterion in the correlation matrix, and subsequent rotation of factors in such a way that all the common-factor variance of the criterion score is taken up by one factor, which is then identified with the principle of classification underlying the criterion. As an example of this approach, we may quote the correlational analysis by Cox of Rorschach scores, taken on 60 normal and 60 neurotic children matched for age, sex, and I.Q.[1] Each score was also correlated with the normal-neurotic dichotomy, which thus became the criterion score in the matrix of intercorrelations. After factorization, axes were rotated in such a way that all the common-factor variance of the criterion score was taken up by one factor, thus identified as "neuroticism," leaving only zero projections for this item on the remaining factors. While this method has certain advantages, it suffers from two great and fundamental drawbacks. In the first place, if the correlations between the individual scores and the criterion score are significant (if, in other words, several test scores discriminate significantly between the criterion groups), then it follows in-

[1] This study was carried out under the writer's direction, and will be published in due course. It should be noted that our comments on this research are not intended as criticisms, as Cox's purpose was not identical with that which we are discussing.

evitably that a factor should be produced from the intercorrelations of the scores which, when rotated in conformity with the principle outlined, would have high loadings for the criterion score. In other words, it is doubtful if the factorial approach adds anything of fundamental scientific interest that would not be given equally well, and more quickly, by some form of multiple regression or discriminant function analysis.

The other objection is even more fundamental; the procedure outlined begs the question which is really the ultimate justification for the factorial quest. We assume that the criterion groups are situated along a continuum which constitutes what Cattell calls a "source trait"; this fundamental assumption cannot be proved by means of the procedure described here. The fundamental assumption that neuroticism is a source trait remains an assumption, and if, as the writer believes, it be true that the main *raison d'être* of factorial methods lies in their ability to prove or disprove fundamental taxonomic questions of this kind, then clearly the method of rotation through an outside criterion is not of great general importance. It assumes, as do all the orthodox statistical methods dealing with the significance of differences, or the maximizing of such differences (analysis of variance and covariance, discriminant function analysis, multiple regression, etc.), that the main dimensions, or source traits, have already been located, and that contrasting groups, representing extremes along these dimensions, have already been located. Factor analysis alone sets out to discover which are these main dimensions, and it is precisely this feature which constitutes its claim to serious consideration.

Having rejected the method of external criteria, we must now turn to the method of internal criteria, *i.e.*, the methods of simple structure and of proportional profiles. There is such a large body of discussion dealing with these principles that we shall merely indicate with extreme briefness why we consider that they also fail to solve the problem which factor analysis sets out to attack. We shall not enter into such points as the question of invariance or uniqueness of the solutions offered; it is realized fairly widely that simple structure solutions are not unique (*i.e.*, different psychologists analyzing the same matrix would not emerge with identical solutions), and they have not been proved to be invariant. These matters are vitally important, of course, but we would lay stress rather on a different aspect of these proposals which appears even more open to criticism. Thurstone and Cattell make the assumption that if some "general guiding principles" could be arrived at from an analysis of the matrix of intercorrelations itself, then we would be ensured of finding "factors corresponding to realities." Thurstone phrases this point rather differently by saying that only when factors are rotated in conformity with his principles do they become "psychologically meaningful." It is this fundamental assumption underlying the work of both Thurstone and Cattell which appears doubtful to the present writer. At the very least it would appear to require some form of proof; clearly as a principle it is not self-evident, as is shown by the fact that many experts, Burt and Thomson among them, have expressed views seemingly in contradiction to it. Yet no such proof is attempted, nor is it at all easy to see precisely how it could be given. It would almost seem as if the principle of simple structure, and that of proportional profiles, were engaged on a gigantic game of tautological hunt-the-slipper, in which artificial statistical rules applied to a matrix or a set of matrices are supposed to give reliable

and valid information about real psychological influences.

III. Criterion Analysis

Any statistical method of analysis is appropriate only to certain types of problems. The type of problem to which criterion analysis is appropriate may be described most easily by reference to an actual investigation. On the basis of a number of experimental and statistical investigations described elsewhere (2), the writer has advanced the heuristic hypothesis that there exists "a general factor of neuroticism, similar in mode of derivation and general interpretation on the orectic side to the general factor of intelligence on the cognitive side." This hypothesis assumes the existence of some strong, innate tendency predisposing individuals towards definite degrees of emotional adjustment or maladjustment, maturity or immaturity, neurotic or non-neurotic reactivity to environmental stress. It also assumes that the amount of environmental stress suffered by any given individual will affect the likelihood of his actual breakdown. We are not concerned here with the relative contribution of heredity and environment to neurotic maladjustment; what does concern us here is the hypothesis that this putative factor of "neuroticism" forms a quantitative continuum at one extreme of which are to be found hospitalized neurotics, while so-called normals are to be found all the way from the near-neurotic and neurotic to the conspicuously non-neurotic, mature, stable and integrated type of personality.

A second heuristic hypothesis was also advanced, again on the basis of various empirical investigations, to the effect that within the general field of temperament, a general factor of extraversion-introversion could be found which was orthogonal to the factors of intelligence and neuroticism, and which found its prototypes in the neurotic disorders known as hysteria and dysthymia (psychasthenia, neurasthenia, anxiety neurosis) respectively. While this scheme of organization was based on factorial studies, it was recognized that the researches reported did not contain any definite proof regarding the feasibility of the assumptions made. The possibility could not be ruled out that certain qualitative differences existed between normal and neurotic groups, for instance, which gave rise to differences in test scores between these groups; if this were so, the assumption of a quantitative continuum would clearly be untenable.

A similar problem arises in conjunction with a much more widely held theory, namely, that associated with the name of Kretschmer (11). This author believes that there exists a normality-abnormality continuum whose one extreme is not the neurotic, but the psychotic; he also posits that the main factor in the temperamental field finds its prototypes in the main functional psychoses (schizophrenia and manic-depressive insanity), rather than in the neuroses. Instead of a taxonomic system based on neuroticism and extraversion-introversion, Kretschmer therefore has an entirely different system based on psychoticism and cyclothymia-schizothymia. The present writer has outlined Kretschmer's position at length elsewhere (3), and is publishing experimental evidence regarding the adequacy of his theoretical position; here he only desires to draw attention to the fact that both theories (Kretschmer's and the writer's) cannot be right, although they may well both be wrong, and that consequently some form of proof becomes indispensable. Kretschmer has attempted such a proof, which has much methodological interest; the writer has examined it elsewhere, and does not wish to repeat his arguments here; the

conclusion arrived at was that this alleged proof really leaves the issue indeterminate. Clearly what is wanted is a deduction from the hypotheses presented which can be tested by means of statistical procedures; a deduction, needless to say, which is sufficiently precise to avoid an equivocal answer.

Deduction 1: The type of deduction on which our method relies may be illustrated by reference to a hypothetical example in which we are dealing with the "neuroticism" factor, and two tests, T_1 and T_2, which discriminate significantly between a normal and a neurotic group. (We shall leave aside for the time being a consideration of the question of how these groups are chosen, or of problems of sampling which arise.) On the hypothesis that neuroticism is a continuous variable, a "normal" group would include persons differing in degree of "neuroticism." Now clearly the more highly "neurotic" subjects in the normal group should have higher "neurotic" scores on the two tests than the less highly "neurotic" subjects (we are assuming here that the tests have a threshold and a ceiling sufficiently far apart to allow differentiation at all levels of "neuroticism," an assumption which will be discussed below). It would follow from this argument that on the average T_1 and T_2 should be correlated in the normal population, a deduction which can easily be verified. (This correlation should of course be purified of the effects of irrelevant factors, such as intelligence, etc.)

This suggested proof could of course be extended to any number of tests; if a battery of n tests discriminates between normal and neurotic subjects, then on the basis of our hypothesis we should expect all the intercorrelations between these tests within the normal group to be positive on the average. We may, however, go further than this and add another specification which also follows directly from our hypothesis. This specification takes into account, not only the fact that our n tests discriminate between normals and neurotics, but also the additional fact that they do so with widely differing success.

Deduction 2: Let us correlate each of our n tests with the criterion, *i.e.*, the normal-neurotic dichotomy, by means of biserial or tetrachoric correlations; we thus obtain a criterion column (C_N) consisting of the correlations of tests $T_1, T_2, T_3, \ldots T_n$ with the criterion. Let us next take the table of intercorrelations between the n tests *for the normal population only,* and submit it to a process of factorization, using either Burt's summation method or Thurstone's centroid method. This will result in the reduction of the large original table of correlations to a small number of factors in terms of which the original correlations can be reconstructed.[2] The actual factors found are purely arbitrary, as their position in the factor space depends on the original selection of tests, conditions of univariate and multivariate selection of the population, and other considerations of a similar kind. Our suggestion for deriving a unique, invariant, and psychologically meaningful solution of the problem posed by this fortuitous structure of factor positions is to *rotate the first summation or centroid factor into a position of maximum correlation with the criterion column,* C_N.

The reasoning behind this suggestion follows directly from the two-test example given earlier. If the fact that two tests discriminate between the criterion groups results in a correlation between the two tests, then clearly the greater the discrimination effected by a test, the higher (*ceteris paribus*) the correlations

[2] Only significant factors should be included in the analysis, of course, using one of the twenty or so available approximate criteria discussed by Vernon (13).

that test will show with other discriminant tests. Similarly, the lower the discriminative ability of a test, the lower will be its correlations with other discriminant tests. Now a factor is the expression of a pattern of intercorrelations existing between a set of tests; if a pattern of intercorrelations such as the one posited here exists in the matrix which is being analyzed, then it should be possible to arrive at a corresponding factor by suitable rotation of the arbitrary factors which emerge from the original analysis. The factor which embodies, as it were, our hypothesis, is of such a kind that the test which best discriminates normals from neurotics would have the highest loadings, while the test which least discriminated between normals and neurotics would have the lowest loadings; the other tests would be intermediate between these two extremes, having factor loadings proportional to the criterion column values. Rotation according to the principle of maximizing the correlation between factor and criterion column would enable us to discover to what extent the hypothesis was borne out; in this sense the principle suggested enables us to use factorial methods as part of the general hypothetico-deductive procedure.

It should be noted that if the hypothesis which is being tested is not borne out by the data, no amount of rotation would succeed in giving us any but a chance correlation between the criterion column and the rotated factors. In other words, the appearance of a high correlation between factor and criterion column may be interpreted as definite support of the correctness of the hypothesis; failure of such correlation to appear is proof of the incorrectness of the hypothesis. These statements are subject to a number of qualifications, which will be discussed below; also the single maximization principle outlined so far must be supplemented and extended by a double maximization principle to which we will turn later. At this stage we shall first give an example to illustrate our method before entering into further theoretical discussion.

IV. Sample Study

In Table 1 are given product-moment correlations between 16 tests which we had reason to believe measured the general factor of neuroticism hypothecated by us, to varying degrees of accuracy; the number of subjects is 64, all of whom were normal in the sense that they were not under psychiatric treatment at the moment, or had been under psychiatric treatment previously as far as could be ascertained.[3] Table 2 gives the first two factors extracted from this matrix by means of Burt's "summation method," grouping of tests being obtained from a preliminary application of Burt's simple summation. Reflection of signs was carried out using an external criterion, namely, the signs of the correlations in the criterion column described below. Reflection of signs was also determined by more usual methods from the matrix itself, using Burt's "inspection technique," and it was found that the two methods agreed in every case. The second factor is significant according to Burt's chi square method at the one per cent level of significance. (Residuals after the extraction of these two factors were insignificant.) Also given in Table 2 are the values for the criterion column, derived from 93 controls and 105 neu-

[3] I am indebted to my colleagues, Drs. Himmelweit, Petrie, and Desai, for permission to re-analyze data collected by them in their work on neuroticism at Dartford; additional data regarding the nature of the groups tested, and the tests employed, will be found in their original publication (6). I am also indebted to Mr. A. Jonckheere, of the Statistics Section of the Psychology Department, for carrying out the statistical work reported below.

TABLE 1

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P
A	—	.009	.043	.118	.157	−.197	−.019	−.049	−.008	−.042	−.017	.061	.229	.299	.095	.196
B	.009	—	.358	.418	−.051	.313	.344	.049	.104	.241	.131	.070	.220	.270	−.001	−.080
C	.043	.358	—	.680	.036	.375	.202	.378	.260	.248	.288	.178	.191	.128	.262	−.071
D	.118	.418	.680	—	−.004	.451	.271	.286	.173	.134	.301	114	256	238	221	008
E	.157	−.051	.036	−.004	—	.387	−.177	.007	.159	.010	.131	.062	.039	.025	−.072	−.058
F	−.197	.313	.375	.451	.387	—	.380	.327	.084	.222	.140	.045	.072	.060	.209	−.027
G	−.019	.344	.202	.271	−.177	.380	—	.062	−.122	.085	.035	−.040	.213	.208	.197	−.055
H	−.049	.049	.378	.286	.007	.327	.062	—	.356	.031	.345	.261	.007	−.013	.154	.316
I	−.008	.104	.260	.173	.159	.084	−.122	.356	—	.252	.315	.346	.292	.294	.147	.029
J	−.042	.241	.248	.134	.010	.222	.085	.031	.252	—	−.073	.105	.253	.417	−.062	.059
K	−.017	.131	.288	.301	.131	.140	.035	.345	.315	−.073	—	.709	.394	.198	.186	.103
L	.061	.070	.178	.114	.062	.045	−.040	.261	.346	.105	.709	—	.450	.410	.167	.273
M	.229	.220	.191	.256	.039	.072	.213	.007	.292	.253	.394	.450	—	.562	.130	.201
N	.299	.270	.128	.238	.025	.060	.208	−.013	.294	.417	.198	.410	.562	—	.178	.327
O	.095	−.001	.262	.221	−.072	.209	.197	.154	.147	−.062	.186	.167	.130	.178	—	.048
P	.196	−.080	−.071	.008	−.058	−.027	.055	.316	.029	.059	.103	.273	.201	.327	.048	—

Intercorrelations of 16 tests of neuroticism; for description of tests see text.

rotics, by means of tetrachoric correlation coefficients. (This normal group of 93 includes the group of 64 on whom our correlations in Table 1 are based; the reason why not all 93 were used for the intercorrelations is that a number of incomplete scores had to be eliminated.) In Fig. 1 are plotted the positions of the 16 tests with reference to the two factor axes.

Below is given a list of the tests used; the scoring in each case is indicated by emphasizing the direction in which the normal group scores as compared with the neurotics. Thus normals are more persistent, less suggestible, more speedy, averagely perseverative, more fluent ideationally, have a higher personal tempo, are more flexible, have better motor control, better dark-adaptive powers, fewer neurotic symptoms on a questionnaire, and show less autism on level of aspiration tests. This attempt to present the tests from the uniform point of view of ***normality*** has forced us into certain awkward ways of phrasing which are difficult to avoid; in the case of intelligence tests this difficulty does not arise as the directionality of such tests is obvious, and recognized in common speech.

TABLE 2

	C_N	F_1	F_2	h^2	$\hat{D}$	$\hat{D}_1$
A	.23	.143	.211	.065	.080	.127
B	.27	.392	−.220	.202	.256	.407
C	.51	.620	−.416	.557	.409	.650
D	.54	.644	−.438	.607	.425	.675
E	.06	.100	.089	.018	.059	.094
F	.10	.497	−.455	.454	.333	.529
G	.05	.275	−.397	.233	.191	.303
H	.57	.405	−.078	.170	.258	.410
I	.30	.438	.175	.222	.267	.424
J	.03	.300	.018	.090	.188	.299
K	.27	.523	.100	.284	.324	.515
L	.17	.565	.461	.532	.333	.529
M	.46	.607	.384	.516	.363	.576
N	.26	.632	.430	.584	.377	.599
O	.24	.294	−.103	.097	.189	.300
P	.21	.207	.241	.101	.119	.189
		.203	.093	.296		

Explanation of Column Heading:

C_N = Criterion Column, *i.e.*, correlation of each test with normal-neurotic dichotomy.

F_1 and F_2 = First and second unrotated factors from analysis of intercorrelations of normal group only.

h^2 = Communality.

$\hat{D}$ = F_1 rotated into maximum correlation with criterion column.

$\hat{D}_1$ = $\hat{D}$ with vector extended to unity

$$r_{\hat{D}\hat{D}_1} = .574$$

$$\rho\, \hat{D}\hat{D}_1 = .587$$

List of Tests

A. Maudsley Medical Inventory—40 item neuroticism questionnaire. Score = number of questions answered "No" (non-neurotic).

B. Dark Adaptation—U. S. Navy Radium Plaque Adaptometer. Score = goodness of dark vision.

C. Non-Suggestibility—body-sway test. Ability to resist suggestion to sway forward.

D. Motor Control—absence of static ataxia; given as preliminary test to C.

E. Goal Discrepancy Score—***smallness*** of level of aspiration scores on O'Connor tweezers test.

F. Judgment Discrepancy Scores—***smallness*** of judgment discrepancies on O'Connor tweezers test.

G. Index of Flexibility—number of shifts in aspiration scores on O'Connor tweezers test, irrespective of size or direction.

H. Manual Dexterity—best score of nine trials on tweezers test.

I. Personal Tempo—speed of writing 2, 3, 4, repeatedly for two trials of 15 seconds each.

J. Fluency—number of ***round things*** and of ***things to eat*** mentioned during 30-second periods.

K. Speed Test (1)—speed of tracing when instructed to be both quick and accurate. (Choice conditions.)

L. Speed Test (2)—speed of tracing prescribed path on track tracer under instruction to be quick.

M. Persistence Test I—length of time during which leg is held in uncomfortable and fatiguing position.
N. Persistence Test B—holding breath as long as possible, without inhaling or exhaling.
O. Stress Test—ability of S to recover previous scoring rate on pursuitmeter type of test after special stress period.
P. Non-Perseveration—extremes of perseveration (SZ test), either very high or very low, are scored low, while scores nearer the average are scored high.

When the correlation matrix in Table 1 is considered, it will be seen that the coefficients arrange themselves in such a way that every single column total is positive; [4] in other words, we find that sixteen tests which differentiate between normals and neurotics intercorrelate (when correlations are run over the *normal* range alone) almost entirely positively; there is not a single negative correlation which is significant at the 5 per cent level in the table, while 39 positive correlations exceed this level, out of 120. (22 of these are significant at the 1 per cent level.) This result may be taken as a confirmation of our hypothesis, as such a large number of positive, and such a dearth of negative, coefficients is unlikely to have arisen by chance. No strict (non-parametric) test of significance is possible, unfortunately, as the correlations are not strictly speaking independent of each other.

It will be seen from Table 2 that the two factors extracted account for 30 per cent of the variance; the contributions being 20.3 per cent and 9.3 per cent respectively for the first and second factors. When we consider that the tests were chosen largely on *a priori* grounds, and that almost half of them fail to correlate even at the $r_t = .25$ level with the criterion, these figures appear very promising. They would certainly be considerably higher if only the most discriminating eight tests had been used in the factor analysis; under those conditions, the percentage of variance accounted for would have risen above the 40 per cent level. Such an exclusion would be perfectly admissible, as it would take place on the basis of an extrinsic criterion, not of one intrinsic to the factor matrix; we have preferred, however, to retain all sixteen tests in our analysis. It is interesting to compare these results with those of a factorial study of ratings carried out on 700 neurotics by the present writer; the contribution of the first two factors was markedly lower than in the present case (2). In view of the fact that inter-personal variability is very likely much greater in a neurotic group than in a normal group, it might be concluded from this comparison that objective tests have a validity which may already be superior to that of psychiatric ratings of the kind used.

We now turn to the most crucial part of this analysis, the interpretation of the factors (Deduction 2). As explained above, the first step in this process involves a rotation of the first factor extracted into maximum correlation with the criterion column.[5] The rotation required is very small, amounting to only 5 degrees. This is understandable, in the present experiment, as the tests were specially selected to define one variable (neuroticism), so that

[4] The fact that every column total is positive would, of course, not be surprising if the signs had been determined by inspection of the matrix, but it will be remembered that the signs were determined independently by means of the criterion column.

[5] More precisely, the two factor matrix was rotated in such a manner that the square of the differences between the loadings on the first factor and the criterion column values were made into a minimum. The resulting first factor of the new matrix was then extended to unit length.

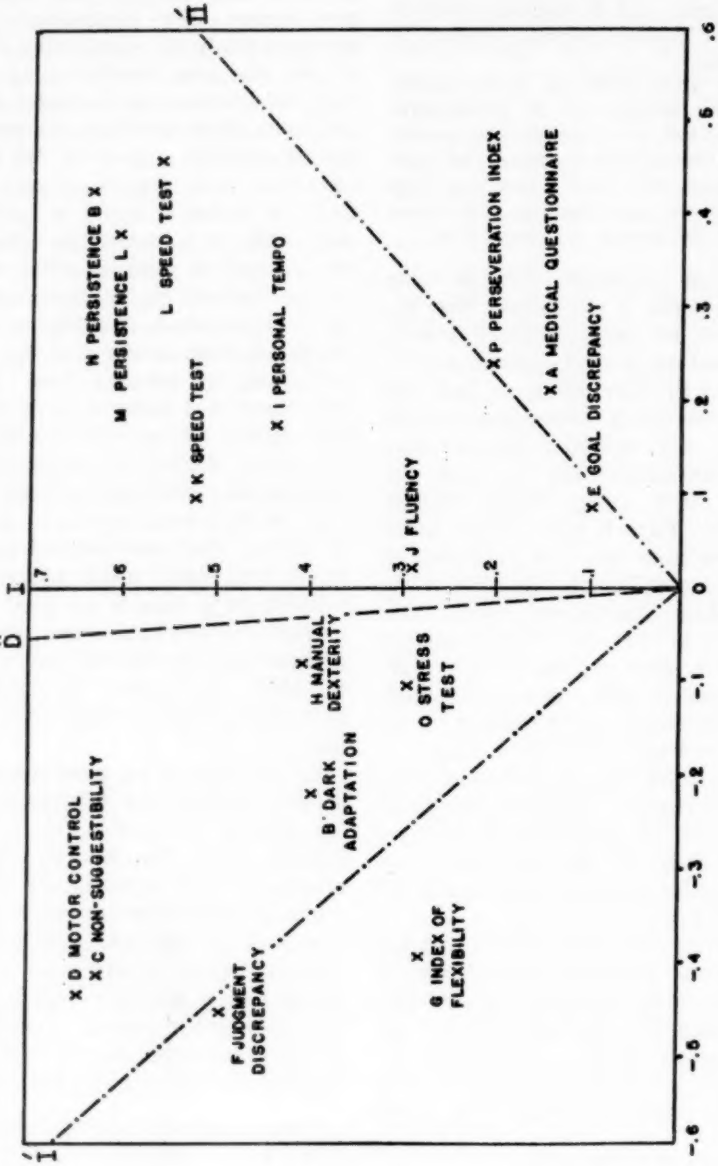
X D MOTOR CONTROL
X C NON-SUGGESTIBILITY
F JUDGMENT DISCREPANCY
B' DARK ADAPTATION
G INDEX OF FLEXIBILITY
H MANUAL DEXTERITY
O STRESS TEST
N PERSISTENCE B X
M PERSISTENCE L X
L SPEED TEST X
X K SPEED TEST
X I PERSONAL TEMPO
X J FLUENCY
X P PERSEVERATION INDEX
X A MEDICAL QUESTIONNAIRE
X E GOAL DISCREPANCY

FIG. 1

the centroid or average of their intercorrelations would not be likely to fall far from the true value. When a larger number of factors is involved, or when a process of univariate or multivariate selection has entered into the sampling of the population tested, the first centroid axis may bear no such close relation to the rotated axis. This finding bears out the writer's previously expressed opinion that the first centroid factor is not necessarily lacking in invariance and psychological meaning, but that interpretation depends in each case on the circumstances surrounding it, and that no general rules of this kind can be laid down.

The correlation between the rotated factor and the criterion column is + 0.574. This value is in the expected direction, and while it is not large enough to be considered definitive proof of our hypothesis, particularly in view of the lack of a statistical criterion regarding its significance, it suggests that further work along these lines should ultimately lead us to a definite conclusion regarding the value of our hypothesis. When we take into account the fact that the great majority of the intercorrelation between the tests are positive, as pointed out above, and that the four tests which from previous large-scale work were known to discriminate particularly well between normals and neurotics (C, D, M, N) have the highest factor saturations in this factor, then we may feel a certain amount of confidence in the ultimate value of the hypothesis under investigation.

The study was not designed to test the hypothesis that an extravert-introvert factor would emerge in addition to the general neuroticism factor, there being no criterion column showing the correlations of the tests with this dichotomy. (Diagnoses of the neurotics enabling us to calculate such a column were not available.) It is instructive, however, to note that the grouping of the tests on the second factor is in conformity with what in previous investigations had been shown to be characteristic of the introvert-extravert (dysthymic-hysteric) dichotomy. Thus introverts (dysthymics) have been shown to be more persistent, extraverts (hysterics) to show less judgment discrepancy, and somewhat less suggestibility, as well as better dark-adaptation. However, not all the tests are in agreement with this hypothesis, and little emphasis is laid on the possible identification of this second factor.

A Thurstone-type rotation into a positive manifold is possible with our two factors, only one insignificant negative saturation higher than − .10 being required.[6] It is our considered opinion that this rotation is psychologically meaningless, and entirely lacks the uniqueness of our suggested solution. In addition, it is irrelevant to the hypothesis which is being tested. It would not be permissible to conclude that "simple structure" solutions would in all circumstances show these characteristics; it is for the adherents of the Thurstonian scheme to indicate more precisely when this scheme is applicable and when not.

V. Discussion

The example quoted above will have brought out certain problems which require discussion. The first of these relates to the question of significance; how can we determine the statistical significance of a maximized correlation between factor and criterion? The answer must be, at the moment, that a strict statistical criterion of significance exists here just as little as it does for the significance of a single factor saturation, or for the residuals on which a factor is based. In most cases it will

[6] This rotation is indicated in the figure, Axes I′ and II′.

be quite clear whether the original hypothesis is borne out or not; however, the absence of a proper method of evaluating significance must of course be regarded as a serious limitation of our method. It is possible, and indeed quite easy, to obtain empirical values which may serve to give an assessment of the chance values to be expected from maximizing correlations between factors and arbitrary criterion columns, and while such determinations cannot take the place of proper statistical derivations, they may help to tide us over until the more rigorous methods are available.

The second problem to be discussed relates to the presence of more than one hypothecated factor in a matrix. Let us assume that we had given a battery of tests purporting to measure both neuroticism and extraversion-introversion to a group of normal subjects; let us also suppose that the same tests had been given to a neurotic group equally divided into hysterics (prototypes of the extravert according to our hypothesis) and dysthymics (prototypes of the introvert). We would then be able to derive two criterion columns, C_N and C_{I-E}, containing respectively the correlations of each test with the dichotomy normal-neurotic, and with the dichotomy hysteric-dysthymic. We would then proceed to carry out a factor analysis of the intercorrelations of all the tests for the normal group, and rotate the first factor found into maximum correlation with the C_N column. The second factor would then be rotated into maximum correlation with the C_{I-E} column, giving either an orthogonal or an oblique angle with the first factor. If we insist on orthogonal relations between factors, we might prescribe that the two correlations between the two factors and the two criterion columns should be maximized simultaneously, giving equal weight to each. If we admit oblique relations, we might make the amount of obliqueness observed a test of the original hypothesis that neuroticism and extraversion-introversion are in fact unrelated. It is impossible to anticipate results in this matter, or in the even more complex problem arising with a larger number of criterion columns; one's decision will be determined by one's purpose, as well as by the exact nature of the data.

The two problems dealt with so far are problems of detail; the third one to be discussed now is one of principle, and of much greater importance. Let us assume that we have selected our neurotic population in such a way that it contains equal numbers of hysterics and dysthymics. Another experimenter might object that obsessional states ought to be included in the typical neurotic group, while yet another might wish to include psychopaths. It must be clear that if the inclusion of different syndromes is based entirely on the whim of the experimenter, we are still far removed from the essential objectivity of a proper scientific procedure. It is here that we may make use of the double maximizing principle. Stated briefly this principle demands that any addition to the main criterion group be permitted only if it increases, or leaves unaltered, the correlation between factor and C column. For example, if it were found that including a number of obsessionals in the criterion group significantly raised the correlation between the neuroticism factor and the C_N column, then we would have objective evidence that obsessional disorders belonged functionally with the hysteric and dysthymic disorders which made up the original criterion group. If psychopaths, when added similarly to the hysteric-dysthymic criterion group, failed to produce a higher correlation between the neuroticism factor and C_N, or even to maintain the existing correlation, then we would have proof that

psychopathy did not belong functionally with the original criterion group.

This double maximizing principle, which is of course widely used in the physical sciences, should enable us to purify the criterion while still following the dictates of the hypothetico-deductive method. The hypothesis that a given clinical syndrome was a neurosis gives rise to the deduction that the addition of persons suffering from this particular syndrome to the existing (imperfect) criterion would raise the correlation between the criterion and the factor; this deduction is capable of being submitted to a crucial test along the lines discussed above. In this way we should be able, starting from a correlation between factor and criterion as low as the one observed in our experiment ($r = .574$), to improve our criterion successively through the addition of new groups until a substantially higher value was reached. This method is not open to objections on the score of subjectivism, and is an important and indeed indispensable corollary to our main principle of criterion analysis; clearly criterion analysis stands and falls with the adequacy of the criterion.

It may be asked whether the maximized correlation could and should reach the value of unity when assumptions are made about a perfect criterion and very large numbers of subjects and tests. It is here that we encounter our fourth problem. The correlation should reach the value of unity if all the tests used had a threshold below, and a ceiling above, the performance of the least and the most neurotic person respectively in the combined populations (assuming for the moment that we are dealing with the trait of neuroticism). If we take as an example the body-sway test of neuroticism, which has been shown to be highly discriminative in comparing normal and neurotic groups (2), we can see from published reports that its threshold is well above the level of the least neurotic person in a combined group, while its ceiling is well below the level of the most neurotic (5). In other words, this test measures only over part of the range. Expressed in purely hypothetical units, we might find that if degree of neuroticism is expressed in terms of the numbers from 1 to 100, suggestibility tests might only be effective measuring devices in the region from 50 to 90, all subjects below 50 falling at or below the threshold of the test, and thus scoring the same mark, and all subjects above 90 falling above the ceiling, also all scoring the same mark. If, now, the dividing line between our normal and our neurotic population happened to be drawn at the 50 mark, it would follow that the discrimination value of this test would be extremely high, but its correlation with the other tests for the normal group alone would be rather low, as its variance would be extremely small and entirely due to error. While this example is of course exaggerated, it does indicate a very real difficulty, and certainly the regressions of each test used on neuroticism should be examined very carefully for linearity in order to safeguard against misinterpretation. There is little doubt in the writer's mind that in part at least the observed correlation's falling short of unity is due to the fact that many of the tests used have thresholds and ceilings which fall within the range of neuroticism measured.

The fifth and last problem to be discussed briefly will be the question of extending the principle of criterion analysis to slightly different types of investigation. As an example, let us take the question of the inheritance of "neuroticism." Along the orthodox lines one method of assessing the influence of hereditary factors would be the administration of a battery of neuroticism tests to monozygotic and dyzygotic

twins, and a determination of the inter-twin correlations within the two groups. These correlations would either be calculated for each test separately, or for some form of summed score. It is suggested that this traditional method does not enable us to extract all the valuable information inherent in the data, and that the use of a modified form of criterion analysis, with particular reference to the hypothetico-deductive method of reasoning, would give us additional information.

In brief, the hypothesis set up would be of the form: A general factor of "neuroticism" exists within the population tested, and *is inherited as a multifactorial unit;* in other words, "neuroticism" is not a statistical artifact, or an arbitrary "principle of classification," but a biological reality. Our method of proof would rest on the maximization of the correlation between the first factor extracted from the intercorrelations between the tests for our subjects, with a criterion column made up of values indicating the apparent influence of heredity on each test separately. Thus if Test 1 showed inter-twin correlations of .5 and .9 for dyzygotic and monozygotic twins respectively, while Test 2 showed correlations of .6 and .8, the values entered in the criterion column would be .56 ($= .9^2 - .5^2$) and .28 ($= .8^2 - .6^2$). To express this method of proof in other words, *the higher the saturation of a test with the factor under investigation, the higher* (*ceteris paribus*) *should be the relative influence of heredity on this test, provided the original hypothesis is correct.* Thus again the correlation between factor and criterion column would serve as a crucial test of the hypothesis.

Many other uses of the principles suggested will be apparent, and there appears little use in discussing them in detail. While in this paper we have dealt exclusively with problems in the field of temperament, there is no reason why criterion analysis should be restricted in this way. The method has already been applied to problems in the field of social attitude measurement (4), and it seems likely that it might be used with advantage in the field of cognitive testing also. Thus, to mention but one example, the hypothesis that certain types of brain injuries, or certain types of therapy (E.C.T.), or certain types of disorder (Korsakoff's syndrome) lead to memory defects could be tested by matching patients falling into one of these groups with a control group equated for intelligence; testing both groups with a battery of memory tests of the kind developed by Ingham (8), and shown to define a factor additional to "g"; developing a criterion column in terms of the differences on these tests between controls and patients; intercorrelating the tests within the normal population; and rotating the first factor extracted into maximum conformity with the criterion column. Again, failure to find an acceptably high correlation would disprove the hypothesis; success in finding such a correlation would support it. Also it should be possible to compare the three criterion columns derived from three different experimental groups (brain injuries, E.C.T., Korsakoff patients), in order to show whether the "memory" defect shown in these cases could be considered to cover the same mental function. One virtue of such a triple comparison would be that only one control group would be needed.

VI. Summary and Conclusions

The writer has tried to show in this paper that orthodox methods of factorial analysis are inadequate for genuinely scientific research because they reject the hypothetico-deductive method which is fundamental to all scientific work. The method of criterion analysis

has been developed in an attempt to imbricate factorial analysis and the hypothetico-deductive method, and an example is given of the use of this new method in relation to the heuristic hypothesis outlined in the writer's book on *Dimensions of Personality* (2). Certain problems raised by the use of criterion analysis are discussed, and suggestions are made regarding the use of this method in a variety of circumstances. It is not claimed that criterion analysis would be a method useful for all types of problems, but it is maintained that for those taxonomic purposes which constitute the primary justification of factorial methods criterion analysis provides a scientifically acceptable and worth-while tool.

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A NOTE ON "CRITERION ANALYSIS"

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The purpose of this note is to:

1) Outline the algebraic procedure followed by Dr. Eysenck in his criterion analysis.

2) Point out the necessity of correcting for univariate selection on the criterion.

3) Discuss the interpretation of the vector that rotates the factor matrix to the criterion column.

4) Prove that criterion analysis will be completely successful when all of the criterion variance that can be predicted from the tests is a function of the common factor scores of the test battery.

I. Notation

Let ${}_jX$ be the q by n matrix of the standardized scores for all the n_j individuals of the jth group on the q tests.

${}_jR$ will be the q by q intercorrelation matrix based on the n_j individuals (${}_jR = {}_jX_jX'$). j can take only the values 1 or 2, since only two groups are considered.

s is the general subscript for a test and can take values $1 \cdots v \cdots q$. ${}_jy$ refers to the column vector of criterion scores for the n_j individuals. This is a set of theoretical rather than observed scores. ${}_j\sigma_s$ refers to the standard deviation of test s based on the n_j individuals.

${}_j\sigma_y$ refers to the standard deviation of the criterion based on the n_j individuals. ${}_jr_{sy}$ refers to the correlation of test s with the criterion y based on n_j individuals. ${}_jr_y$ is the column vector of correlation of the q test with the criterion based on the n_j individuals.

${}_jR = {}_jR_c + {}_jR_u$ where ${}_jR_c$ is the q by q matrix of intercorrelations due to the common factors and ${}_jR_u$ is the q by q matrix due to the unique factors. These matrices are based on the n_j individuals.

${}_jR_c = ({}_jF_c)({}_jF_c')$ where ${}_jF_c$ is the q by m common factor matrix based on n_j individuals.

${}_jR_u = ({}_jF_u)({}_jF_u')$ where ${}_jF_u$ is the q by m matrix of unique factors based on n_j individuals.

Throughout this note it will be necessary to distinguish between statistics calculated within the jth group, based on the n_j individuals; and statistics calculated on the two groups pooled together based on the $N = (N_1 + N_2)$ individuals.

Statistics based on N individuals will *not* have the subscript j preceding them. Thus R refers to the q by q matrix of intercorrelations based on N individuals, and $R = XX'$.

σ_y is the standard deviation of the theoretical criterion scores based on all N individuals. r_y is the column vector of the correlation of all the q tests with the criterion y based on all N individuals. And so on.

We shall define other statistics whenever necessary.

II. The Procedure of "Criterion Analysis"

Dr. Eysenck's computational procedure is as follows:

1) Estimate ${}_jR_c$ from ${}_jR$ by the usual estimations of communalities and factor ${}_jR_c$ into ${}_jF_c({}_jF_c)'$.

2) Find an m by 1 rotation vector t such that $[({}_jF_c)t]$ will have the highest correlation possible with r_y.

Step one can be accomplished by

any of the usual factor analysis procedures. Step two raises the question of how to estimate t. If we define a new m by q matrix

$$_jW = [({}_jF_c')({}_jF_c)]^{-1}{}_jF_c', \qquad (1)$$

then 2) $_jWr_y = t$ is the best (least-square) estimate such that $({}_jF_c)t \doteq r_y$. Estimating t is formally equivalent to Cattell's problem of rotating to a "proportional profile." Ledyard Tucker (4) has used what we have designated as the W matrix for a semianalytical solution for simple structure. In Tucker's notation, our m by q W matrix becomes $P^{-1}F'$. Our m rowed vector, t, would correspond to one column of his L matrix. We shall show later on in section IV that the t vector can be thought of as representing the correlations of the criterion with the m common factors.

III. The Effect of Univariate Selection

It will be noted that in Dr. Eysenck's procedure the correlation matrix is based on the n_j subjects of the jth group. But the validities r_y are based on all N subjects for both groups. In fact the validities are computed using membership in the two groups as a dichotomous score. Now if the criterion is really a continuous variable, and the two groups represent different values of that continuous variable, then using only the members of the jth group for the calculation of $_jR$ is equivalent to univariate selection on the combined group. From this fact it can be shown that the validities $_jr_y$ based on the n_j members of the jth group will not in general be the same as the r_y based on N individuals.

Thomson (3) quotes a formula for the effect of univariate selection on a correlation, based on assumptions of linear regression and homoscedasticity. In our notation,

$$_jr_{sy} = ({}_j\sigma_y)(r_{sy}) \times [\sigma_y^2 - r_{sy}^2({}_j\sigma_y^2)]^{-\frac{1}{2}}. \qquad (3)$$

Thus the new validity $({}_jr_{sy})$ within the jth group will be a function of the criterion standard deviation within the jth group $({}_j\sigma_y)$ and the validity (r_{sy}) and criterion standard deviation (σ_y) of the N individuals.

The correlation between any two variables s and v within the jth group could also be calculated.

$$_jr_{vs} = \frac{[r_{vs}\sigma_y^2 - r_{sy}r_{vy}(1 - {}_j\sigma_y^2)]}{\sqrt{\sigma_y^2 - r_{sy}^2(\sigma_y^2 - {}_j\sigma_y^2)}\sqrt{\sigma_y^2 - r_{vy}^2(\sigma_y^2 - {}_j\sigma_y^2)}}. \qquad (4)$$

If we wish to transform r_y into $_jr_y$ then all we need do is calculate the diagonal matrix $_jP$ whose q diagonal elements are $[\sigma_y^2 - r_{sy}^2({}_j\sigma_y^2)]^{-\frac{1}{2}}$ and perform the multiplication

$$_jr_y = {}_j(P)(r_y)({}_j\sigma_y). \qquad (5)$$

In equation (5) P is a q by q diagonal matrix, r_y is a q by 1 vector, and $_j\sigma_y$ is a scalar. It is obvious that $_jr_y$ will not be proportional to r_y unless the diagonal elements of $_jP$ are all equal. The diagonal elements of P will be equal if and only if every validity is equal, *i.e.*, if

$$r_{1y} = r_{2y} = \cdots r_{sy} = r_{vy} = \cdots r_{qy}.$$

Since in the general case the validity of the q tests will be different, $_jr_y$ will not be identical with r_y. To be consistent with the fact that the intercorrelation matrix $_jR$ is based on n_j individuals, it would seem that $_jr_y$ should be substituted for r_y in Dr. Eysenck's procedure for criterion analysis.

It will be recalled that we have no observed criterion scores within the

jth group. We cannot compute the validities ${}_jr_{sy}$ in the same way that ${}_jR$ was computed, by using actual scores. Instead the univariate selection formula (3) must be used to solve for ${}_jr_{sy}$. A glance at equation (3) shows that all the independent variates are known except one, ${}_j\sigma_y$, the standard deviation of the criterion scores within the jth group. But since the theoretical criterion scores of each of the ${}^nj$ members are unknown, we must use the univariate selection formula (4) for ${}_jr_{sv}$ to estimate ${}_j\sigma_y$. In equation (4) for ${}_jr_{sv}$, only ${}_j\sigma_y$ is unknown. It should be possible, though rather cumbersome, to use the known value of ${}_jr_{sv}$ to solve for ${}_j\sigma_y$ and then substitute this estimate of ${}_j\sigma_y$ into the formula for ${}_jr_y$.

IV. The Meaning of the Rotation Vector "t"

Let us assume that having performed a factor analysis of the q tests in the jth group, we wished to obtain the factor loadings of the criterion y on the m common factors. Let us further assume that we do not wish to introduce any new communality due to the correlation of the unique portions of the q tests with the criterion variable. P. S. Dwyer (1) and C. I. Mosier (2) have published the solution to this problem. Their solution for the vector representing the factor loadings of y on the m common factors is algebraically identical with the solution for t that we have presented in this note.

$$t = ({}_jF_c{}_jF_c)^{-1}{}_jF_c({}_jr_y) = {}_jW({}_jr_y). \quad (6)$$

Theorem I

The rotation vector t will be equal to the correlation of the criterion y with the m factors when the correlations of the unique portions of the q tests with the criterion are zero.

Proof: Let ${}_jP_c$ represent the m by n_j matrix of standardized common factor scores such that

$${}_jX_c = ({}_jF_c)({}_jP_c).$$

Let ${}_jP_u$ represent the q by n_j of standardized unique scores such that

$${}_jX_u = ({}_jF_u)({}_jP_u),$$

where

$$\begin{aligned}{}_jX &= {}_jX_c + {}_jX_u \\ &= {}_jF_c({}_jP_c) + {}_jF_u({}_jP_u).\end{aligned}$$

Postmultiplying the q by n_j matrix of standard scores ${}_jX$ by a column vector, ${}_jy$, representing the n_j standardized criterion scores, we obtain

$$\begin{aligned}({}_jX)({}_jy) = {}_jr_y &= ({}_jF_c)({}_jP_c)({}_jy) \\ &\quad + {}_jF_u({}_jP_u)({}_jy). \quad (7)\end{aligned}$$

But if the correlations of the unique portions of the q tests with the criterion are zero, then

$$({}_jP_u)({}_jy) = 0, \quad (8)$$

and

$${}_jr_y = ({}_jF_c)({}_jP_c)({}_jy). \quad (9)$$

By equation (6) premultiplication of ${}_jr_y$ by ${}_jW$ will give us t. We obtain

$$\begin{aligned}&[{}_jF_c{}^1({}_jF_c)]^{-1}{}_jF_c({}_jr_y) \\ &\quad = t = ({}_jP_c)({}_jy). \quad (10)\end{aligned}$$

Therefore it has been shown that when the correlations of the unique portions of the q tests with the criterion are zero, the rotation vector t equals the correlations of the criterion y with the m common factor scores.

Corollary to Theorem I: When there is no unique variance in any of the q tests, the rotation vector t is equal to the correlation of the criterion y with the m common factors.

Proof: Given: ${}_jF_u = 0$. Substituting zero for ${}_jF_u$ in equation (7) we obtain equation (9). Premultiplying by W to get t, equation (10) is again obtained. Therefore, when the unique variance vanishes, t is equal to the correlation

of the criterion with the common factors.

It will be noted that from equation (9) we can state that

$$_jr_y = ({}_jF_c)({}_jP_c)({}_jy) = ({}_jF_c)t. \quad (11)$$

This is a statement of algebraic identity. It is *not* a question of estimating $_jr_y$ from $({}_jF_c)t$. This result can be stated in a form which makes use of Theorem I and its corollary.

Theorem II

When the correlations of the unique portions of all tests with the criterion are zero (including the case when all unique variances vanish), then criterion analysis will be completely successful, *i.e.*, a vector t can be found such that $({}_jF_c)t$ is exactly equal to $_jr_y$. When $_jF$ is a q by q matrix, then it is always possible to reproduce $_jR$ exactly. From this it can be shown that $({}_jF)t$ and $_jr_y$ are always algebraically equivalent.

Theorem III

When the q by q factor matrix $({}_jF)$ is obtained such that $_jR$ is exactly equal to $({}_jF)({}_jF')$, then t is identical to the correlations of the criterion with the q factors and criterion analysis will be completely successful.

Proof: $_jR$ is the q by q correlation matrix. It is given that $_jR = {}_jF({}_jF')$. The formula for the matrix of q by n_j standardized scores can be written in terms of $_jF$ and the q by n_j matrix of standardized factor scores, $_jP$.

$$_jX = {}_jF({}_jP). \quad (12)$$

Postmultiplying $_jX$ by the set of n_j criterion scores $_jy$, we find that

$$X_y = {}_jr_y = {}_jF({}_jP)({}_jy). \quad (13)$$

Premultiplying $_jr_y$ by W to get t we find that t is equal to the correlation of each of the q factor scores with the criterion $t = ({}_jP)({}_jy)$. Therefore it follows that when $_jF$ is a factor matrix which reproduces R exactly, a t can be found such that $_jr_y$ is exactly equal to $({}_jF)t$. It should be noted that this is always possible since $_jF$ can be a matrix with as many factors as tests.

SUMMARY

1. It has been shown that r_y (based on N individuals) will in general differ from $_jr_y$ (based on n_j individuals).

2. It has been shown that $_jr_y$ and $({}_jF_c)t$ will be algebraically identical when:

(a) there are no unique factors $({}_jF_u = 0)$;

(b) the correlation of the unique scores with the criterion is zero, $[P_u({}_jy) = 0]$;

(c) when $_jF_c$ is a matrix such that $_jR$ is reproduced exactly, $({}_jR = {}_jF_j F')$.

This last condition can always be met by using as many factors as there are tests.

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MOLAR AND MOLECULAR

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Introduction

Psychology has had more propensity than most disciplines to react to concepts as if they were good or bad. Today, for example, it is good to be dynamic, functional, operational, field-theoretical and molar; it is bad to be static, structuralistic, non-operational, atomistic and molecular. The good terms seem to produce a feeling of expansiveness, while the bad ones may be symbolized by a cramped feeling in the face of a confusing multiplicity. Since one can hardly deny the scientist his status as a personality, we may expect the affective components of theory to be with us always. Indeed, some have said that new theories are but a leap from one set of affects to another. But it is also true that the constant reconsideration of affectively charged words is a major technique of scientific advance.

We propose to consider two of the above terms [1] because they represent a rallying point for certain recurrent arguments in systematic psychology. The terms are *molar* and *molecular*. We will attempt to show that the central role they have achieved in psychology has been magnified and distorted by the various ways they have been interpreted. Before reviewing these different usages, a brief glance at their historical setting may prove useful.

A major emphasis in recent methodology is that theories are contingent and that scientific description may take many forms. This view of theories as alternative rather than necessarily contradictory proposals implies that they may be differentiated in terms of the *units of description* they employ. By using these units as criteria of communicability between theories many conflicts have been resolved. Explicit recognition of this approach appeared most clearly in psychology with the publication of Tolman's *Purposive Behavior in Animals and Men* (15). The reader need hardly be reminded of the eagerness with which the terms molar and molecular were then adopted. They quickly entered the substance of most psychological theory and only one major protest was heard. Koffka (10) objected to the generosity with which Tolman located the molecular in a systematic hierarchy. For Tolman every level of the hierarchy is equally legitimate, so that, in a sense, molecular and molar are equally important. For Koffka, on the other hand, the levels have a relationship of priority so that the molar is more general and basic.

The general favor with which the distinction was met is quite understandable. Up to that time much research in the applied areas of education, social psychology and personality—not to mention other disciplines such as soci-

[1] Throughout the paper, *term*, *concept* and *construct* will be used interchangeably. Our inquiry does not require a more specialized use.

ology or anthropology—had been characterized by a certain amount of guilt and defensiveness. Nowhere could one point to the physiological basis of this research, to the real stuff of behavior, as it were. As the applied psychologists hastened to point out, however, consistent and discriminating variables *were* being isolated and defined. While most of the research in learning at this time also lacked organic substantiation, guilt-feelings were avoided by virtue of an historic orthodoxy and a readiness for neurological speculation. This lack of sureness with respect to the necessity for physiological explanations had led to a sharp conflict between two frames of reference, the biological and the psychological. Tolman's efforts to resolve this conflict represented a convergence of many trends: Gestalt psychology; comparative psychology in the persons of Hobhouse, Thorndike, Washburn and Yerkes; the behaviorisms of Watson, A. P. Weiss and Kantor; social psychology and personality study in the related views of McDougall and Freud; and the philosophy of Holt, James and other pragmatists. In a sense Tolman was the final step in establishing the legitimacy of the psychological approach. The unrespectability of much earlier work could be seen, in the light of Tolman's analysis, to have arisen from a disparity between great subtlety with respect to psychological phenomena and a lack of sophistication with respect to operational or investigatory procedures. Accepting the psychological framework of these non-biological theorists,[2] Tolman insisted that such a framework be related to data in a clear-cut, communicable fashion.

Another way of looking at Tolman's contribution is to say that he united certain features of positivism and emergentism. The emergentist stressed that organic matter has different levels of functioning which emerge out of the organization of phenomena, and that these *new* levels are real and require their own descriptive devices. Tolman, on the other hand, stressed the ***descriptive*** aspect of the problem, observing that in addition to the physiological descriptions of organic life, there were possible other descriptions which appeared to be at least as fruitful. Phrased in this way the distinction between levels of functioning becomes neither metaphysical nor existential but methodological. Tolman's approach, then, was to find out what questions could be made systematically meaningful within a purely psychological framework. He argued that a scientific theory has meaning in so far as it is based upon a set of observable events, and that such meanings are a function of the way these events are described—regardless of the kind of event investigated. One way of looking at events, he further suggested, is in terms of whether they can be described as part or total functions. These functions were termed molecular and molar, respectively. The distinction provided a rationale for separating physiology from psychology by labeling physiology as a part process. But Tolman was not too exact in specifying what he meant by part or whole functions and, consequently, the mean-

[2] We are aware that many of the people we have labeled psychologically oriented, *e.g.*, Freud, McDougall, Washburn, were highly trained in the biological sciences, and that they were at all times cognizant of the "animal" nature of their subjects. What we are emphasizing is that their data and much of their theory were purely behavioral. While all expressed belief in the "dependency" of mind on body, they did not feel guilty about discussing mind; the belief in this dependency gave them permission to emphasize one side of what was for them an equation. Those psychologists who could not calmly accept this equation, and wished to retain the psychological point of view, were the ones who found themselves in the anxious state we have described.

ing of molar and molecular was left somewhat indefinite and a variety of interpretations arose. It is to these interpretations that we now turn.

We list below seven different meanings which we have arbitrarily named. The first three are propositional; that is, they are explicit definitions of the terms. The remainder are implicit; they have not been formally defined, and operate as programmatic, affective usages in psychological thinking. The reader will note that authors who find the terms molar and molecular useful do not usually specify which of these many meanings is meant. Indeed, most frequently, they intend several of them simultaneously. Univocality is the exception.

MEANINGS

1. *Interaction.* Molar has been used to refer to experiments or observational series employing many variables, treating them in interaction with each other. The analysis of variance design would seem to be molar in character. Contrariwise, the classical experiment of one independent, and one dependent, variable in isolation from all others, would appear to be molecular. This type of analysis implies a continuum of molarity-molecularity. Thus the t statistic used to evaluate the classical type of experiment is but a special case of the F statistic. However, writers who identify molarity with interaction usually mean something other than this statistical tying together of independent entities. They emphasize rather the interdependent nature of the very definitions of these variables, and the necessity of describing events as a matrix of factors. Thus Brunswik, in his *Outline of History of Psychology* (3), distinguishes between molecular and molar as a difference between an approach which is inadequate, fragmentary, over-simplifying, bit-by-bit, atomistic, and microscopic, losing sight of the whole, as contrasted with an approach which is adequate, encompasses the essentials of macroscopic, uses dynamic units ("*e.g.*, of the personality as a whole, of behavior-patterns"), and properly considers coherence and the role and interaction of parts within a context. Brunswik's contrast overlaps some of our later meanings, but his emphasis is on isolated variables vs. comprehensive, interacting variables.

2. *Action-units.* This meaning has become increasingly prominent. A relatively long-time unit is molar, a short-time unit is molecular. The molar unit of description is a behavior episode bounded in time by *initiation* of behavior by a need, and *cessation* of behavior by achievement of a goal (Muenzinger, 13). It is not time-length *per se* that is emphasized, but rather the meaningfulness of behavioral description in terms of some functional unit. Thus Krech and Crutchfield say,

> "In the absence of molar units, the description of behavior can be little more than an enumeration of unsystematized bits and pieces of momentary, limited, and unrelated responses. Viewed wholly, in the context of needs and goals, on the other hand, the behavior of the individual can be seen as meaningfully organized. The unity implied in the molar description is not something arbitrarily imposed by the psychologist in viewing the individual as he behaves; the individual is a dynamic unity, a whole person, and it is as such that he takes part in social phenomena" (11, p. 31).

It may be noted that this passage reflects Gestalt philosophy as to the nature of the world: molar units are necessary because people *are* molar. The world *is* organized in molar fashion, and the molecular-molar argument is one of content as well as methodology. But not all advocates of such long-time units have tied their conception of behavior to the Gestalt notion of an ar-

ticulated universe, as have Krech and Crutchfield. Muenzinger advocates these start-end units as the only way of breaking into and handling the fluid, Jamesian stream of events.

3. *Levels*. Psychology is molar, physiology and neurology are molecular. A response is molar, a group of muscle twitches is molecular. Hull (8) explicitly distinguishes between molar and molecular in this way, and we have mentioned Tolman's equation of physiology with part processes. But in the two previous definitions of molar and molecular the physiologist also can be molar. A study of endocrine balance may have many interacting variables, and use long-time units. It seems, then, that it is not physiology that is molecular, but rather physiological explanations of psychological phenomena. Such explanations are obviously reductionistic. The question then becomes, "Is reductionism necessarily molecular?" As judged by some of the above criteria of molarity, a glance at classic physiological explanations of behavior would seem to answer this question positively. The physiological variables used in the past to account for behavior have in the main been simple and momentary—neural bonds, reflexes, twitches, etc. But it would be inapplicable to call isomorphism or Lashley's (12) network theory of brain function molecular; and the future may witness molar attempts at physiological explanation of behavioral phenomena. The theories of Rashevsky (14) and Wiener (17) are illustrative of this possibility.

4. *Construction*. The genotypic is considered to be more molar than the phenotypic or peripheral because it is assumed to possess a greater degree of generality and explanatory power. This usage involves construction of a set of abstract intervening variables considerably removed from peripheral data and presumably accounting for them. A high degree of abstraction is of course possible in dealing with surface variables alone, if by abstraction is meant a rational generalization, verbal or mathematical, which does not merely duplicate or summarize data. But all generalizations are not necessarily molar in the present sense. Hecht's (6) work, and much of Hull's may serve as illustrations of phenotypic, nonmolar generalizations though in other respects Hull functions as a molar theorist (*e.g.*, the concept of $_SH_R$); when contrasted with the "central layer" abstractions represented by, say, a Thurstone factor, many of the variables of Hecht and Hull would be classified as molecular. In contrast with our first three meanings, the present one is usually an informal coupling rather than a formal definition, for one does not find explicit equation of molar and genotypic in the literature.

5. *Phenomenal*. This usage is in marked contrast with the previous one, for the *phenomenal* criterion equates molarity with treatment of data as given. The emphasis is upon the appearance of the variables and the process by which they are observed. The phenomenal or untutored is molar while the derived or analytic is molecular. Katz's (9) phenomenological varieties of color may be contrasted with the structuralists' description of color sensations.

Lifelike in contrast with artificial is a characteristic often associated with this sense of molar.

Indeed, in this as in our next usages, there is stress on the need for reproducing, by means of scientific procedures, the richness of organismic functioning. There is discontent represented by Goldstein (5) and by Allport (1) with the partialling of life by classical science. The present authors believe that this motivation has been important in generating the first three,

more formal definitions. The historical background of the concept of molarity, discussed above, may be viewed as an unfolding of this need for realness. The same motivation plays a part in the *construction* usage: intervening variables are often used as a bridging device for a ready return to lifelike phenomena. There is a great difference, of course, between equating molarity with phenomenal data and equating it with intervening constructs to handle data; yet the motivations of such diverse approaches seem to have a common core.

6. *Urgency.* Here molarity-molecularity has shifted completely from a level of methodological relationships to one of value decisions. If the psychologist's description is believed to be applicable to problems considered significant by society, then it is molar.

Classification according to this category is dependent on belief as to what is important in pure and applied psychology. In so far as psychologists disagree on the relative importance of specific psychological problems, various approaches will be differently classified as molar or molecular. The distinction arose at a time when general psychology was returning to the task of incorporating purposeful behavior into its framework. Present-day concerns with social and clinical problems have reaffirmed the need for such an endeavor.[3]

7. *Holism.* The more consideration there is of the organism as a whole or the unity of the organism, the more molar is the approach. Explicit description of a part of the organism is molecular. Though this criterion seems to stem from Tolman's original contrast of part and whole processes in learning, the distinction has frequently become purely verbal and has been based on the degree of lip-service to wholeness and unity. Indeed, holism as a philosophy antedates Tolman, and many present-day holist philosophers have merely borrowed the term molar from him.

DISCUSSION

We shall consider first the interrelationship of the meanings, and then the contemporary significance of the molar-molecular distinction.

1. Certain problems arise when we attempt to interrelate our seven meanings. One may first examine the question of concurrent use of several meanings. Another passage from Krech and Crutchfield will illustrate the not unusual[4] simultaneity of meanings to be found in the literature:

"PROPOSITION I

"The proper unit of motivational analysis is molar behavior, which involves needs and goals.

"Of first importance in our analysis is the decision as to the units of behavior to be employed. Shall we be concerned with mere simple, segmental activities such as muscle twitches, movements of the limbs, swallowing, sweating, swearing, and the like, or shall we be concerned with total *behavior acts,* such as getting married, vot-

[3] The category of urgency brings into focus a paradox. While many of Tolman's principles, usually considered more molar than Hull's, are believed to be applicable to problems of human learning, the latter's have so far found a wider application; for example, in the social-psychological and personality studies of Miller, Dollard, Kluckhohn or Masserman. We are not attempting, however, to evaluate the extent to which Hull's principles have *succeeded* as explanatory concepts in dealing with social and clinical problems. Rather, we are merely noting a current *historical* event.

[4] A number of other authors could have been used as illustrations here, for example, Barker, Wright and Gonick's (2) treatment of molarity. We have selected passages from Krech and Crutchfield because they have verbalized so clearly the significance which molarity has for them. The clarity of their presentation makes it possible to tease out many implications of the problem previously operating on an implicit basis.

ing for a political candidate, participating in a lynching? The former is a 'molecular' unit, and the latter a 'molar' unit."

"The distinguishing features of molar behavior are (1) that it includes all the behavior of the individual occurring at the same time (his needs, emotions, thoughts, perceptions, actions, etc.) and (2) that it consists of relatively discrete, unified episodes with a beginning and an end. . . . The beginning of Mr. Arbuthnot's lunchtime molar behavior is the onset of feelings of hunger; the end of the episode comes with the completion of eating and the feelings of satiety" (**11**, pp. 30–31).

In the above quotation, molar seems to mean *action units, urgency,* a great number of simultaneous variables (presumably interacting), and *holism* ("*all* the behavior of the individual"). Implicit in such a quotation is the assumption that these four meanings, by an inner logic, are interdependent, standing or falling together. The question is not whether *these* particular meanings are in actuality necessarily inconsistent but whether, as a general rule, the construction of a proposition containing many dimensions of meaning is an advisable procedure. The danger is that if one component of the proposition is negated the entire complex may fall.

Associated with lack of independence in the usage of molar-molecular is a lack of clarity with regard to some basic assumptions: Does molar refer to the *size* or the *qualitative* nature of events? Is molarity methodological, referring to a way of describing, or existential, referring to some thing described? Are its referents internal to psychology, or are they external and inter-systematic? These problems contribute to much of the confusion found in the literature. They highlight the difficulties brought about by multiple meaning. We shall discuss them one at a time.

(A) *Quantity vs. quality.* Many of the usages apparently deal with units of *description,* as illustrated by *holism* which equates molarity with large units. The *construction, phenomenal* and *levels* criteria emphasize the *kind* of unit. *Action-units* seem to require units of a certain kind and also large units. Distinctions between quantity and quality are always dangerous, but there does seem to be a difference between emphasizing the descriptive *inclusiveness* of a unit as contrasted with the *kind* of unit adequate to describe behavior. Which of the two emphases is intended by molar can be clear only if its meaning is specified.

(B) *Methodological vs. existential.* The criteria of molarity often seem to imply that the *subject matter* of psychology is of a special kind, and therefore requires a special manner of description. Behavioral events *are* molar, so the description, too, must be molar. The criterion of *action-units* often suggests this but even more so does the criterion of *holism.* Though the latter has in the main been unable to specify what units should be chosen, it rejects thoroughly descriptions of part processes; this springs apparently from a conviction that the organism *is* a unitary whole, not merely that it can be *described* as a unitary whole. To the degree that these criteria are existential, they seem to clash with Tolman's original stress on molarity as an instrumental property. The other criteria have in the main retained the original instrumental aspect: It is molar to describe phenomena in a certain way; it is not the content described which is molar or molecular.

The writers feel that existential criteria propose a metaphysical criterion for an empirical procedure. By empirical we mean that events may be described in various ways, one of which is in terms of a distinction between molar and molecular. If desirable this dis-

tinction may even be given substantive status. Such a step, however, serves only to define the *locus* of investigation and cannot be considered as a statement about the events that are investigated. Description and ontology should not be confused. In the long run scientific systems appear to be judged by some characteristic such as fruitfulness, rather than by coherence with any given ontology. Perhaps science has tolerated ontologic speculation because it seems to serve the purpose of breaking a mold of thought, a direction habit, so that while the heuristic and propaganda value of theories about the real nature of events is indeed valuable, it appears never to be relevant to any empirical system. Value and ontology act as catalysts, and are presumably social or cultural instruments that define directions rather than specify relations.

(C) *Internal vs. external referents.* The criteria of *levels* and *urgency*, in contrast to the other usages, refer to more than units. They characterize systems, inter-disciplinary relations and applications as molar or molecular in nature. It is not the variable that is molar, but either the hypotheses made, the type of explanation employed, or the use to which the variables can be put.

The criterion of *levels* is unique in contrasting psychology with non-psychological neurology, physiology, etc. While the other usages localize the distinction between molar and molecular within the realm of psychology, the substantive character of neurology and physiology is used to define their molecularity. It is of some significance that this distinction has almost always dealt with interdisciplinary relations in only one direction. That is, there has been little systematic analysis of relations between anthropology, sociology, economics, political science, history and psychology. Often it is implied that these other fields are not to be considered within this framework of classifying behavioral phenomena. Such a position, however, is difficult to maintain, for if the *level*-type of distinction is a substantive one, then any independent research area (defined as a field where investigatory variables may be isolated) demands inclusion within this classification schema. On the other hand, if there exists a characteristic of greater or lesser molarity, the term molecular loses most of its meaning and the substantive distinction loses its heuristic value. Tolman has recognized the problem and offered his solution to it in an interesting review (16).

2. Of what significance are these rather scholastic distinctions for the contemporary scene? Let us re-emphasize the fact that the molar-molecular distinction entered psychology at a time of crisis. Its usefulness in pointing a way out of the schisms that rent the psychology of the time was great. It provided a rationale, a justification for an approach that has borne much fruit and it legitimized new paths of investigation.

With the achievement of this task, however, the role of the molar-molecular distinction has shifted, perhaps to the point of losing its usefulness. Today one seldom finds the question of legitimacy raised with respect to research of a non-physiological nature, the investigation of purposeful behavior, or the consideration of aspects of organismic functioning previously slighted. The frame of reference has changed. The specific contribution made by the distinction has been almost universally accepted so that the multiplicity of meanings we now find may be an index of future unproductiveness. With this multiplicity there has arisen an affective attachment to the terms, *qua* terms, which serves to cloud the problem and dominate fresh approaches.

We suggest, therefore, that the terms be abandoned, for it does not seem possible to purge them of their confusing connotations.[5]

Our analysis, therefore, complements those of theorists who have noted a process of convergence in the last twenty years of psychology, a trend toward dissolution of the schools of psychology (Brunswik, **4**; Hilgard, **7**). Interest in systems has changed to interest in functional relations and systematization. In this shift the distinction between molar and molecular played the vital role of releasing psychology from premature and confining systems. But in so doing it diminished its usefulness and achieved the status of ground rather than figure in current theory.

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[5] Dr. Tolman has indicated that he shares this opinion as to the future role of molar and molecular (personal communication).

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1901	8	1	2	–	–	–	–	$1.00	$2.00
1902	9	–	2	–	–	–	–	$1.00	$1.00
1903	10	1	2	3	–	–	–	$1.00	$3.00
1904	11	1	–	–	4 &	5	6	$1.00	$4.00
1905	12	1	2 &	3	4	5	–	$1.00	$5.00
1906	13	–	–	3	4	5	6	$1.00	$4.00
1907	14	1	2	3	–	5	–	$1.00	$4.00
1908	15	–	–	–	–	–	–	—	—
1909	16	1	–	3	4	5	6	$1.00	$5.00
1910	17	1	2	3	–	–	6	$1.00	$4.00
1911	18	1	2	3	4	5	6	$1.00	$5.50
1912	19	1	2	3	4	5	6	$1.00	$5.50
1913	20	1	2	3	4	5	6	$1.00	$5.50
1914	21	–	2	3	4	5	6	$1.00	$5.00
1915	22	1	2	3	4	5	6	$1.00	$5.50
1916	23	1	–	–	4	5	6	$1.00	$4.00
1917	24	–	–	–	4	–	–	$1.00	$1.00
1918	25	–	2	3	4	5	6	$1.00	$5.00
1919	26	1	2	3	4	5	6	$1.00	$5.50
1920	27	1	2	3	4	5	6	$1.00	$5.50
1921	28	–	2	3	4	–	6	$1.00	$4.00
1922	29	1	–	–	4	–	–	$1.00	$2.00
1923	30	1	2	3	4	5	6	$1.00	$5.50
1924	31	1	2	3	4	5	6	$1.00	$5.50
1925	32	–	2	3	–	5	–	$1.00	$3.00
1926	33	1	2	3	4	5	6	$1.00	$5.50
1927	34	1	2	3	4	5	6	$1.00	$5.50
1928	35	1	2	3	4	5	6	$1.00	$5.50
1929	36	1	2	3	4	5	6	$1.00	$5.50
1930	37	1	2	3	4	5	6	$1.00	$5.50
1931	38	1	2	3	4	5	6	$1.00	$5.50
1932	39	1	2	3	4	5	6	$1.00	$5.50
1933	40	1	2	3	4	5	6	$1.00	$5.50
1934	41	1	2	3	4	5	6	$1.00	$5.50
1935	42	1	2	3	4	5	6	$1.00	$5.50
1936	43	1	2	3	4	5	6	$1.00	$5.50
1937	44	1	2	3	4	5	6	$1.00	$5.50
1938	45	1	2	3	4	5	6	$1.00	$5.50
1939	46	1	2	3	4	5	6	$1.00	$5.50
1940	47	1	–	3	4	5	6	$1.00	$5.00
1941	48	1	2	3	4	5	6	$1.00	$5.50
1942	49	1	2	3	4	5	6	$1.00	$5.50
1943	50	1	2	3	4	5	6	$1.00	$5.50
1944	51	–	2	3	4	5	6	$1.00	$5.00
1945	52	1	2	3	4	5	6	$1.00	$5.50
1946	53	1	–	3	4	5	6	$1.00	$5.00
1947	54	1	2	3	4	5	6	$1.00	$5.50
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